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AN ANNOUNCEMENT

It is with deep regret that announcement is made of the retirement of Professor Titchener from the editorship of THE AMERICAN JOURNAL OF PSYCHOLOGY, effective with the last issue.

Psychologists universally will acknowledge that under his direction the JOURNAL has reached its highest usefulness and distinction, and that it has occupied a place among the very best existing scientific periodicals.

The JOURNAL will be continued by the persons whose names appear below, who will try to maintain it at the high standard set by Professor Titchener. Communications and manuscripts may be sent to any one of the four editors.

MARGARET FLOY WASHBURN
KARL M. DALLENBACH
MADISON BENTLEY
EDWIN G. BORING

MECHANISM, MEANING AND TELEOLOGY IN BEHAVIOR

By Arthur Ernest Davies, Colorado College

In a former article,[1] I pointed out that behaviorism is historically connected with the frank acceptance of a biological point of view in the study of human and animal life. The statement of Watson that behaviorism is the only consistent functionalism, is an inadequate recognition of the truth of this view. It is inadequate because the functional psychologist never overcame the dualism of mind and matter that is an inheritance from the Cartesian philosophy of the 17th century, and he only succeeded in masking the subjectivism that is inseparable from this point of view by falling back upon a theory of parallelism of body and mind, or upon the more speculative double-aspect theory. As a result, functionalism merely asserted that the idea of development may be applied separately to the body and the mind. It then became the business of the psychologist, (1) to show that in the latter, as in the former, there were functions of increasing complexity, (2) to describe these, and (3) to point out their inter-relations. Moreover, the distinction between body and mind committed the functionalist, when dealing with the mind, to the method of introspection, and, when dealing with the body, to the method of objective observation. This reliance upon introspection by the functionalist should have warned Watson of the error in his statement of the historical relations of behaviorism. It was suggested, in the former article, that the connection of behaviorism with genetic psychology was much more intimate and exact. In support of this position, it was pointed out that genetic psychology had two independent starting-points and lines of development: the one, definitely biological, in the study of animal behavior, and giving rise to what is known as comparative psychology; the other, biological in its methods and aims, in the study of the mind of the child and of the so-called 'primitive' races. In both these movements, illustrated in part by the work of Jennings, Thorndike, and Mark Baldwin, introspection, as commonly understood, was thrown aside; and instead of asking what takes place within the organisms in question, they sought to ascertain what selected organisms actually do in definitely controlled conditions. The first task of genetic psychology, therefore, was the description

[1]The Influence of Biology on the Development of Modern Psychology in America, *Psych. Rev.*, 30, 1923, 164-175.

of behavior, and behaviorism might with a good deal more accuracy be said to be the systematic application of the principles and methods of genetic psychology.

Now within these more general relations, it is to be expected that the particular methods of approach to behaviorism should exhibit among the members of this school variations of numerous kinds. In the case of Watson, for example, three somewhat distinct motives may be made out. There is, (1) a decided reaction against the traditional psychology which shows itself in his rejection of introspection, and his repudiation of anything in the nature of 'philosophy' in dealing with the problems of animal and human life. Sometimes in reading Watson one is led to wonder if he has escaped the danger that lies in all reaction whatever and has, as the Germans say, "thrown out the baby with the bath water." This would, of course, be the case if, as many who are not at all unfriendly to behaviorism are saying, his rejection of mind or consciousness is for the psychologist mistaken. The question that is raised by this remark is not whether, as the founder of a new science, he is within his rights in delimiting its enquiries in any way he saw fit—no one, I think, would dispute this point; the question rather is whether the rejection of mind or consciousness in the sense of a spiritual principle or substance necessitates its repudiation in any sense whatever. The criticism might be made that he has failed to do in a systematic way what, for example, Locke felt it incumbent upon him to do in regard to innate ideas, namely, subject the notion of mind or consciousness to a critical examination in order to ascertain if there is any sense in which the notion can be intelligently used.[2] It is true that Watson has shown in a

[2]I may remark here that I agree entirely with Woodworth that the behaviorists' attitude toward consciousness is quite unsatisfactory, and is likely to remain so as long as it is not subjected to a rigid and careful critique. I find an illustration of this unsatisfactory state of affairs in Weiss' misunderstanding of a remark of mine in the summary of the article previously referred to. What I said was: "I do not see that behaviorism, so long as it remains on scientific grounds, can be criticized for starting with the reality and observability of behavior; but if and when it discusses the mental order, either as something that does or does not exist, it is open to the criticism of raising a question which, because the answer to it must ultimately rest upon an appeal to consciousness, undermines its own position." The point was, of course, that behaviorism has the choice of two courses. First, it may confine its studies to behavior in the sense of neuromuscular reactions, and then the question of consciousness is not raised. But if, in the second place, it speaks of consciousness at all, it must do so in some traditional sense, or in some sense that it is to be expected it is able to define. Now whichever of the latter alternatives it chooses, the possibility of mutual understanding, I insisted, rested upon experience of the kind of thing that the word connotes.

I may add that because he did not see that I was leaving the question of the meaning of my consciousness open, Weiss says, "my statement is

number of his writings, first, that the notion of mind or consciousness is a shifting and unreliable one; and secondly, that it is possible to deal systematically with some of the problems usually connected with this notion without having recourse to introspection. But this hardly meets the case. So long as he remains in the paradoxical position, first, of denying all reality to mind; and secondly, of saying that if it exists it has no interest for the psychologist, there will be reasonable grounds for supposing that his reaction against traditional opinions has exercised an extreme influence upon the way behaviorism has developed in his hands.

(2) The second source of Watson's behaviorism is his acceptance of the methods and aims of biological science. There are many who are not Watsonian behaviorists who are in agreement with him at this point. His book on *Behaviorism* illustrates well enough the positive motives to assimilate psychology without remainder to biology. The necessity of this attempt, in the extreme form in which it appears in Watson, seems to many to lie not so much in the warnings of history, in the lack of development of the science, say, from Aristotle to Wundt, but in Watson's own reaction to that history, in his rejection of the traditional subject-matter of the science.

(3) The third motive is illustrated in his *Psychology*. In this book, behaviorism ceases to be, or to be primarily, what it was in the other, a body of doctrine, and functions definitely at a "standpoint." It is, of course, possible to make too much of this distinction, but the difference in content and emphasis of the two books are the outcome, for Watson himself, of the different motives that prompted their writing. I should say that the spirit that determined the work of genetic psychology is more in evidence in the latter book, and the value of it lies in showing that the objective methods of the biological sciences may be expected in competent hands to be serviceable in the study of the problems of life and mind.

The positive influences that define Weiss' relations to behaviorism are in the main of a different sort. Except in the general recognition of the biological character of behaviorism,

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meaningless to those who deny the existence of consciousness *in any traditional sense in which the word is used,*" (Italics mine.), and he says this because he *assumes* that I was using the word consciousness as "*that something which* specifically may be characterized as redness, etc." It is interesting to note that in this paragraph in which Weiss undertakes to state his attitude toward consciousness, he arrives at the conclusion that there is no such thing, that is to say, he accepts the first of the two main alternatives in the quotation from my article, but he does so on the ground that one of the traditional definitions is not acceptable, without inquiring if any other definition is possible. That is to say, he ignores the second alternative, and, therefore, has carried us no further than we were at the beginning.

there is little in Weiss' exposition that is in either the spirit or the method of Watson. One way of stating the reason for this would be to say that Weiss is not as competent a biologist as Watson. It would, however, be truer and more to the point to say that he has brought to the service of behaviorism a special training in mathematics and the mathematical sciences. From this point of view, it was almost inevitable that he should say, as he does, that, however far we may be at the present time from a satisfactory or complete understanding of behavior, the psychologist must regard it, as the scientists regard all other phenomena, as a form of motion. The laws of behavior, when they come to be written will be specially complex formulae of the type that, in more simple forms, we have become acquainted with in mathematical physics. This view, as we shall see, imports into behaviorism a definitely mechanistic conception and raises many questions that, at present, we cannot answer. I think, for example, that it is because he has taken this position that Weiss is brought into agreement with Watson in the attitude he takes toward mind or consciousness. In assimilating psychology to physics he is led to the denial of consciousness as inevitably as is Watson when, from more complex motives, he assimilates psychology without remainder to biology. I shall return to this point later. A second distinguishing characteristic of Weiss' behaviorism is his admission of the usefulness of philosophy in defining and elucidating the problems of this type of psychology. In his earlier relations to behaviorism, it was philosophy in the form of epistemology that was chiefly emphasised. That this was so was not surprising, for he was merely following an historic tradition that stretches back to the physicists of the 16th. and 17th. centuries. Questions about the nature and conditions of human knowledge, its certainty and validity, seem not to be remote when specifically physical problems are under consideration; and the more theoretical physics becomes the more the need is felt for a statement of the grounds on which its conclusions rest. This ground, of course, can be nothing else than human knowledge itself. In the latest systematic statement of his views, however, Weiss leaves his former love and boldly embraces metaphysics, on the ground that it is only by so doing that scientific development can be secured for behaviorism.[3] This seems to me unfortunate, first, because it leaves us in doubt as to whether he has receded from his former position; and secondly, because it introduces into his behaviorism a major premise, physical monism, that appears unduly to influence the points of view and particular doctrines that he is led to adopt. This is the least satisfactory feature of his two articles,

[3]Behaviorism and Behavior, *Psych. Rev.*, 31, 1924, 35.

and I have not been able to rid myself of the impression that certain questions would have received a more empirical and less theoretical treatment if he had avoided the delusive promise that, no less for the behaviorist than for the rest of us, is inseparable from the premature introduction of theories of reality into the work-shop of the scientists.

If now we consider the course we have taken, it will be evident that students of behavior are confronted, now from one point of view and now from another, with three special problems. In the first place, behavior, from the standpoint of the mathematical sciences, is determined, and it calls for a mechanistic explanation. In the second place, from the standpoint of biology, it possesses survival-value, and thus raises the question of meaning. In the third place, from the philosophical and especially epistemological point of view, behavior, if it is creative or has meaning, can hardly avoid being thought of as having some relation to an end.

I propose to say something on every one of these points, but before I do so I shall state my own attitude toward the movement or series of movements that have crystallized under the name behavior. I do this in order to dispose, at the start, of the possibility of anyone thinking that I am prompted by any other motive in what I may say than the desire to look at the facts that traditionally belong to psychology in an unprejudiced way, and to find a formulation that summarises the facts themselves. It has seemed to me a pity that criticism is so often left to those who belong to another party or group, and that it consequently assumes the form of a polemic. Immanent criticism, a criticism of particular positions and the positions of particular writers, from the standpoint of the antecedents and larger relations of the subject, might lead us nearer to the removal of contradictions, to a recognition of internal inconsistencies, if any, of principle, and to a systematic development of doctrine. Behaviorism owes this to itself and to those to whom they would recommend whatever it is that goes, or is to go, by the name behaviorism. Unless this is done, there is danger that, as in the similar case of pragmatism, some one will write an article on the 'thirteen kinds of behaviorism.' This would be unfortunate if it led to an indefinite postponement of the recognition of what is common amid all the variety of doctrine that is unavoidable in the working out by individuals of the fundamentals of this branch of psychology.

I hasten, therefore, to say that I am not opposed to the concept of behavior as applied to the subject-matter of psychology. Indeed, before Watson challenged current conceptions I had already, in 1901, formulated a definition of psychology that was definitely behavioristic in statement and intention. At the time, I thought of writing this out for wider circulation, but, from the point of view I then held, it seemed to me such an obvious thing to say as to be lacking in novelty. Psychology was defined, in the notes referred to, as "the systematic study of the adjustments of psycho-physiological organisms to their environment," and the effort was made to give a definite content to each of the major terms of the definition. I do not think, of course, that this definition would be accepted by all groups of behaviorists at the present time, but twenty years ago there was better hope of this, and even now all would probably agree that the conception is definitely behavioristic. As I have not seen any reason to depart from the notion then formulated, it may be taken as a statement of my relations to behaviorism, and of my present attitude toward its various current developments.

I pass now to the topics proposed for discussion in this paper. I shall consider (1) behavior and mechanism; (2) behavior and meaning; (3) behavior and teleology. A question not far away is, "Is there any way to reconcile mechanism and teleology on behavioristic principles?" An affirmative answer would have to be given if it could be shown that behavior displays both determined and undetermined characteristics, and if certain questions involved in the notion of cause could be satisfactorily disposed of. I postpone this question because it seems to me important enough to receive separate consideration.

I. Mechanism in Behavior

I wish to state my attitude toward an assumption that seems to affect the particular form and emphasis of a good deal of contemporary behavioristic discussion. In order to introduce the particular occasion that gives rise to the assumption in question, it is necessary to divide 'psychologists' roughly into three groups: (a) those who regard "consciousness, as such" as the legitimate subject-matter of their science; (b) those who define the materials of the science in terms of behavior, meaning by that term the activities, neuro-physiological or chemico-physical, that constitute the adjustments of organisms to their environments; (c) those who combine the points of view of (a) and (b) in some sort of workable, or unworkable, relation. There are, that is to say, the right and left wings, and the middle-of-the-roaders. I have included the third group not merely for the sake of completing a very tentative classification, but in order to say that I shall not refer to them again in this paper, because most, if not at all, who belong here seem not to have realized the very fundamental nature of the questions that have been raised by behaviorism; and because I think these questions can be discussed with any degree of clearness only when they are approached from the standpoint of the more radical contrast presented by the positions of the first two groups. When we consider this contrast it seems to be taken for granted by both sides that the main issue is definitely whether mechanism has any place in, or is a characteristic of the facts considered by psychology. On the one side, mind or consciousness is repudiated because it has not lent itself to mechanical formulation; on the other, behavior is looked upon with suspicion because it is, in principle, capable of reduction to nothing but mechanism. Because of these negative views, it has come to be assumed by both parties that the case for consciousness is bound up with an indeterministic position of such an extreme character that all order, regularity, or law is, in the nature of the case, excluded. Thus, in order to oppose this indeterminism the behaviorist is led to over-emphasise mechanism, and in order to oppose mechanism, the mind psychologist is led to over-emphasise in-

determinism. All this seems to me unfortunate in the extreme, and one of the consequences of it, among the behaviorists themselves, is Weiss' refusal to admit to the world of existence,—and that means, as I take it, the world of reality,—anything that falls outside what he calls the "movement continuum." One would have thought that questions of reality were sometime in their discussion questions of fact, and it is the question of fact that I find assumed in a good deal of behavioristic discussion. Whether 'consciousness' could or could not sustain a rigid examination as to its factual existence is not now being considered; what I am saying is that the importance and even necessity of making such an examination has been obscured by reason of the position that behaviorists and others have taken with respect to questions that have no necessary relation to this one. Mechanism, I have pointed out, is one of these, the "movement continuum" is another.

I am quite prepared to be told that in taking this position I am entirely mistaken. For is it not true that a great deal of effort and ingenuity have been expended by the behaviorist in showing that consciousness in the form of images or what not is merely a set of non-conventionalized motor patterns that may differ from individual to individul depending upon the particular social and cultural conditions under which the individuals in question have grown up? But this is beside the point. For the question I am raising does not refer to the kind of explanation behaviorism has to offer of images and so forth, but to the terminology in which such experiences are most accurately described. What a good many persons are interested to know is if behaviorists have the kind of experiences that most of us speak of as images, ideas, etc., for unless they have there is lacking a common ground upon which discussion can proceed. Behaviorism seems to have balked at this point because it has unwarrantably supposed that in order to speak of consciousness at all one has to mean a "*something which* specifically may be characterized as redness, sweetness, affection, awareness, envy, etc."[4] That is to say, as respects its conception of

[4]An attempt to determine consciousness in more objective fashion is made by W. S. Hunter in his article on "The Problem of Consciousness," *Psych. Rev.*, 31, 1924, 1-31. With respect to the question of comparing what is in my mind or consciousness with what is in another person's, we shall, of course, be told by the behaviorist that this is wholly a matter of the "speech reaction." I experience three difficulties in accepting this explanation. The first is that it is not an answer to the question raised. What, for example, we ordinarily mean by an image is the apprehension of a material object when that object is outside the range of our perceptual, that is, receptor-effector system. Does the behaviorist have to be in physical contact with the object in order to describe and react to it? Or can he describe and react to it in its physical absence? If he can, how is this possible unless he apprehends it in some non-physical fashion? The second difficulty is that the speech reaction is not a primary, but a secondary or acquired relation to things. Nothing that we know certainly warrants the view that below thought our relations to objects, material and personal, are conditioned by developed language. Thought is so conditioned, but it has also a phylogenetic relation to percepts and images. The domain in which the two latter function does have a decided and controlling effect on behavior, on the ways in which the neuro-muscular system adjusts itself grossly to its environment. If, for example, the primitive man has an hallucination, say, of a ghost, he does not make a speech describing what he sees, he runs away as fast as he can and shuts himself up in his kraal. Afterwards, when he has recovered from his fright, he speaks about it, and words begetting other words amplify, develop, and obscure the original occurrence. Do the behaviorists propose to substitute in this region of man's life mythology for science through the unregulated use of the speech reaction? A third difficulty appears when we ask what, for a mechanistic science, speech is. The answer has to be, a certain number of air waves that have an individual configuration. Is not this a survival, under a disguised and more scientific form, of Democritus' 'eidola' that floated off from one individual to another making inter-individual cooperation possible? I may add that there seems to me a good deal that might be said for Woodworth's contention that the speech reaction of the behaviorists is really nothing but the old-fashioned introspection in disguised form. This however would take me too far afield.

consciousness, behaviorism is held in thrall by the 17th. century Cartesianism, and seems quite unable to suppose that any other meaning of the word is possible.

It would, I think, induce to clearness if we dropped the word "consciousness" altogether and spoke of the mental order in somewhat the same way as, referring to facts of a particular kind, we now speak of the physical order of events. It would then be possible in an empirical way to collect specific instances under each category, to describe their characteristics, and to ask ourselves whether, and if so to what extent, the mental order is or is not capable of the same kind of explanation as the physical. The differences between the mind psychologist and the behaviorist would then refer to the latter consideration, assuming, of course, that we were able to come to a common agreement as to what the characteristics are that justified us in classifying an event as mental or physical. Such a procedure however, reasonable as it would seem, is precluded by the behaviorists when they accept dogmatically a philosophical theory about the world, physical monism, for example, which prejudices from the start the attitude they are led to take toward facts that belong to the mental order. If this method is justified on the part of behaviorism, I do not see why, accepting, for example, a theory of spiritual monism, the mind psychologist can be criticised for saying that the "movement continuum," the whole physical order of events, is as epiphenomenal as the behaviorist now contends the mental order of events is. My opinion is that we shall get nowhere in either case, and that it would be better to adopt a strictly empirical attitude in our study of the problems of life and mind.

Accepted as a controlling point of view for the study of behavior, the mechanistic position compels us to classify the questions involved in the investigation of the so-called secondary qualities as fictitious. On this basis, there are no secondary qualities, and, therefore, no genuinely scientific questions that can be formulated in regard to them. Yet, let it be said, a very considerable portion of the time and energy of a good many of the leading minds in this country and in England, during the last twenty years, have been given to the discussion of just such questions. In fact, the modern form of the realistic controversy may be said to center upon the status of the secondary qualities. Quite apart from the answer that has been given, by different groups of realists, to this question,—from the contention that secondary qualities are physical, mental, or neutral entities,—it is obvious that the evidence by which either or all of these positions is supported is ignored by the behaviorist when he places his doctrine on some such general argument as this: 'Everything that is real is physical; secondary qualities, if they cannot be ignored, are real; therefore, secondary qualities are physical.' It is nothing to the point to say that this conclusion agrees with the position of one of the realistic schools, for, first, the evidence of the other schools, so far as it is evidence, must be taken as throwing doubt upon the validity of the conclusion in question; and, secondly, it is not at all likely that the new realists would agree that there is any justification for identifying, as the behaviorists seem to do, the conditioned (the secondary qualities) with the condition (the neuro-muscular organism), that is its invariable antecedent.

It may be pointed out that the attempt to resolve, by the magic of an accepted philosophical theory, what exists into its series of antecedent conditions is to repeat, in a new connection, the mistake of historical empiricism, and to dissolve the whole substantial world itself, the "movement continuum" to wit, into

the accidental configuration that is known as the individual. This is the necessary logic of the position of Weiss,—and he states it in so many words. He writes: "When the behaviorist begins to formulate the facts which he is to include under the term behavior on the assumption that the universe is the totality of the electron-proton aggregates and the changes that occur in their spatial relationships, he may begin his analysis in the following concrete manner: Observing an infant which he must regard as an electron-proton aggregate, he finds that the child's visual and auditory sense organs are being exposed to practically the same electron-proton conditions as his own and the adults around it. Yet its responses are entirely different. If for instance the child's nurse responds to the position of the hands of the clock by preparing the infant's bath, the infant if it responds to the clock at all will do so by cooing, crying, listening posture, etc. Unless the infant responds to the clock in some specific manner, there is no way for the behaviorist to determine whether that electron-proton aggregate known (to the behaviorist) as a clock, is modifying that aggregate known as the infant. Further, *both clock and infant are only specific movement effects produced in that aggregate known as the behaviorist.*"[5] This is as clear a statement of solipsism as one could wish; and, I submit, there is no real choice between spiritualistic and materialistic solipsism. We have reached a position where logic has gone bankrupt, and it is only a matter of taste with which variety of the doctrine, if any, one prefers to be identified.

The advantage of approaching the problems of behavior from the standpoint of biology, rather than from the standpoint of physics, is that we are not thereby committed inevitably to the theory of mechanism. It is true that one of the aims of science is prediction and that prediction implies uniformities of some sort, but this may be granted without begging the question as to the kind of uniformities that are involved in human and animal behavior. Watson has given a general statement of what psychology would be if and when the behavioristic programme is carried out. He says: "A psychology of interest to all scientific men would take as its starting point, first, the observable fact that organisms, human and animal alike, do adjust themselves to their environment by means of hereditary and habit equipment. These adjustments may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly, certain stimuli lead the organism to make responses. In a systematic psychology completely worked out, given the responses the stimuli can be predicted; given the

[5] *Op. cit.*, 40. Italics mine.

stimuli the responses can be predicted."[6] Such a statement, Watson admits, is "crass and raw in the extreme," but I think it must be admitted that in so far as it emphasises "the necessity for maintaining uniformity in experimental procedure and in the method of stating results in both human and animal work," it must be accepted as fairly expressing the scientific aim in this field of inquiry. The statement also has the advantage of leaving open the question of the specific relation of stimulus to to response, and reserves for separate consideration the nature of the mediating links between the two. In other words, Watson is here insisting that scientific psychology is primarily concerned with experimentally determining a set of correlations between stimulus and response, and that when this has been done we have all the ground that science can provide for inferring the response when the stimulus is given, and the stimulus when the response is given. It is true that Watson believes that all behavior can ultimately be explained in terms of a physico-chemical interpretation of the connection between stimulus and response, but one would have to be very forgetful of the present-day situation of biology to say that the acceptance of a biological point of view commits the psychologist to the position of materialistic determinism.

Finally, it is of importance, from the biological point of view, to indicate a certain feature of behavior that is likely to be overlooked when problems of explanation are under consideration. Explicit behavior, those forms of adjustment by which an organism adapts itself in overt action to changes in its environment, has a significance beyond the spatial and temporal relations it happens to sustain to the physico-chemical alterations that occur within the organism in question. We may, for example, be able at some future date to write a series of physico-chemical formulae that express the changes that take place within the organism when certain types of adjustment to the environment occur; but from such a series of equations we shall never be able to infer the value of the adjustment in question for the species to which the individual belongs. This reference of the activities of an individual to the continuity of the species is what the biologist means by survival-value, and it would seem evident that a complete account of behavior requires us to interpret it not only in terms of the mechanisms of reacting organisms, but also in terms of its mediate values. But if we say this, we have already passed beyond the mechanistic point of view, and are confronted with the second topic of discussion, namely, behavior and meaning.

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[6] *Psychology from the Standpoint of a Behaviorist*, 1919, 10.

II. Meaning in Behavior

The problem of meaning is a difficult one. It need not, however, be made more difficult than it is by failure, on the part of those who discuss it, to distinguish between the statement of the problem and the proposed solution of the problem. In the light of this distinction, no one, I presume, would claim that we have reached a satisfactory understanding of the mechanism of meaning, and he would be a very courageous or ignorant man who would claim that there is agreement on all sides as to what the problem is. If we contrast, for example, the "escort of images and ideas" of the older psychologists with, say, Stout's "conative continuity," we catch a glimpse of the differences that have emerged among the mind psychologists about the problem in question; and if we contrast with either or both of these Weiss' "biophysical and biosocial equivalence," we have a fairly good measure of the distance we are, at the present time, from agreement on a fundamental and important question in psychology. Now these differences in doctrine, it would be absurd to deny, do go back to differences in methods and points of view of those who expound them; but a more minute comparison of the doctrines would, I think, reveal the fact that they are also in part dependent upon a difference in the way the common problem has been conceived by the several writers. That is to say, the problem of meaning has a larger or narrower denotation at the discretion or convenience of the person who discusses it; it is a variable category that narrows itself to the problem of how we perceive things, or that enlarges itself to include all the problems that are embraced in the fact of our most advanced culture,—the social, economic, political, aesthetic, moral, religious behavior and experience of the race. I am no wiser than my peers, and, therefore, do not propose for myself the rôle of deciding, amid this diversity of usage, where praise or blame, if either, belongs. The humbler task belongs to me to point out that there is implied a common fact, namely, the fact of complication, and the question that is raised by this fact is, what account we are able to give of it.

Complication in human experience and behavior is only another name for development and, if we so take it, it may not be difficult for all of us to agree that a primary condition of any profitable discussion is that the problem be broken up into a number of constituent questions, and that each be given a careful definition. Failure to do this adequately, I think, has led, and is likely to lead, to a good deal of unprofitable discussion; and, moreover, it has allowed to creep into current views assumptions of a quasi-philosophical sort that, if they are to be admitted at all, should be introduced by name, along with the other facts and factors of the problem, to the rest of the company. At present, they are in the position of the man "without a wedding garment," and when detected immediately become suspect. One such general assumption is that all complications are of the same general character, and that, therefore, when we have studied one we have, in principle, studied all. This, of course, may be so, but I do not know how

we are to be certain of it in the absence of a classification of the particular complications we meet with in experience and behavior, and a detailed study of each. There can be no objection to this procedure on the part of the scientific behaviorist or psychologist, and if it is suggested that 'meaning' is a problem, one of a number of other problems, that arises in the effort of animal and human organisms to adjust themselves to their environment, the question, perhaps, had been cleared of a number of possible misunderstandings.

Mechanism in behavior was conceived and discussed in the last Section as a question that involved a study of the changes that take place in the receptor-effector apparatus with which the organism is endowed; it resolved itself into a study of behavior as consisting of modes of activity that were necessitated by inherited structures. The only question at issue is what the nature of the mechanisms is by which such behavior is brought about.

In this Section, in discussing 'meaning,' I shall have in mind chiefly the relation of behavior to the stimulating conditions by which it is made possible. Before taking this subject up in detail, I want to remove, as a possible source of difficulty, a whole class of facts from the limits of the discussion. It is, for example, agreed that the fundamental mechanisms, whatever their nature, by which behavior is mechanically determined have been evolved in the history of the species *pari passu* with the evolution of the environment to which these mechanisms enable the organism to adjust itself. In this way, we may imagine, a relation has been established between certain conditions in the environment and certain conditions in the organism, such that when the former act on the latter there results what is called behavior. We are here in the field of what the biologist calls heredity. I am not prepared to say what the limits of heredity in this sense are, nor am I able to say, if it has limits, what its upper range is. All that is necessary to state is (a) that there is a sufficiently close correspondence between environment, in this field, for us to approximate roughly Watson's ideal of prediction; and (b) that we reach here the closest resemblance, in the whole range of behavior, to what may be called mechanism. I have said that I want to exclude all this from the present section, although here too the question of the correspondence of behavior to its stimulating conditions is involved; but, I think, the facts themselves and the demands of scientific method require the separation of behavior in this sense from behavior that does, or seems to, involve 'meaning.' However, it is not to be presumed that behavior in this latter sense is independent of the mechanisms that are at the basis of behavior in the former sense. The only question is of their adequacy; or, to put it differently, the problem of meaning arises when the environment presents a complexity that goes beyond that which is characteristic of the situations in which the fundamental reaction-patterns have evolved. From this point of view, behavior is said to have meaning when the adjustment has some obvious and statable relation to this complexity. The study of meaning, therefore,

is a study of the mechanisms by which what is new in the conditions of life is met by behavior that goes beyond the limits of heredity. The question underlying this study is whether behavior ever exhibits this characteristic. This would seem to be the case.

I shall take as my first illustration what is commonly referred to as a perceptual situation. This is made up of a number of physical objects, some of which, let us say, are confined to given spaces, and some are changing their spatial relations to other objects and to one another at varying rates. There is, in this situation, a framework of stable objects and, within this framework, an indefinite number of other objects that change from a state of rest to one of motion, and others from a state of motion to one of rest. Whatever the variable details in the two classes of objects may be, the situation has a certain configuration, and may be said to have a measurable duration. In order to make the situation thus described concrete, let us say it is fairly realised in a city street. If we introduce into this situation an individual who wishes to pass from one side of the street to the other, it is obvious that while his behavior has a determinate relation to the situation as a whole, the more prominent features of it will consist of the variations that occur in passing from one side of the street to the other, in the sudden stoppings and darting forward as he threads his way among the moving objects that constitute for him the important features of the total situation.

There is, of course, nothing new in saying that in such a situation the individual's movements are to be classified as behavior, or that the senses, so far from giving him detailed information about the nature of the objects to which the individual reacts, simply enable him to adjust himself to these objects in a self-preservative way. This doctrine of perception was formulated as far back as the 17th. century.[7] In modern terms, this is to say that behavior refers to a situation which lies outside the neuro-muscular changes to which the mechanistic behaviorist would reduce all behavior whatever, and it is contended that the neglect of this feature in his descriptions obscures for the behaviorist an important problem in his explanation of how behavior is possible. A city street we ought to remember, even if we are psychologists, is the product of the industrial revolution, and the practical situations it presents are surely not among the conditions under which our remote ancestry developed. Its psychological features are as new as its economic, and require, as urgently as the latter, the development of new methods of coping with the questions that it presents, as, for example, our "Safety First" campaigns attest. New patterns, or new combinations of patterns, are a necessity of the situation; but in any case the resulting behavior, if it survives, will show a direct relation to the conditions under which it evolved, and to this extent it will have meaning.

There is, I should say, nothing novel in this contention. For the motor patterns that figure so conspicuously in behavioristic psychology, that is to say, our inherited stock of 'neurograms,' to use a term of Morton Prince with a somewhat different meaning, are themselves the product of the influence of environment on the development of the nervous system, and carry within themselves, that is to say, in the configurated pattern of motions that is characteristic of each of them, a reference to the conditions under which they too have developed. Hence, in

[7]Malebranche, *Recherche de la Verité*, Bk. 1, Ch. 5.

order that a motor pattern may become functionally active in an organism, we have to suppose that a situation exists that is essentially similar to the series of conditions under which, in the history of the species, this pattern has developed and with reference to which it has become standardized. Moreover, it is presumed that different patterns have developed under different conditions, and therefore embody and express different meanings.

If we accept this view of the neurological factors involved in behavior, is it possible to give any account, other than the one suggested, of those explicit movements, coördinated with the motor patterns, by which the organism adjusts itself to its environment? Do not these also refer to the environment in a way that justifies us in regarding them as functions of it? Whether, therefore, our perceptual situation be an old and familiar one, or a new and strange one, an analysis would seem to show that the behaviorist has over-simplified his account of behavior, and that because he has done so he has failed to recognise that at the very heart of behavior lies, for behaviorist and psychologist alike, the problem of meaning.

I shall take another illustration in order to bring out certain ambiguities that lurk in our current use of the word environment, and that affect our description and explanation of behavior. I take the infant-nurse-clock example mentioned by Weiss.[8] Weiss states that all the individuals in the room are "exposed to practically the same electron-proton conditions." This is either a theoretically harmless assertion, or it is a practically vicious one. It is harmless if it is taken to mean that a description of the room can be given that would not be altered by the coming and going of anyone, infant or adult, who at the time happened to be in the room. It is moreover harmless if we are to understand by the "exposure" of the individuals to the room nothing more than their compresence with it. The statement would become immediately vicious if, as seems to be the case, it is supposed that whatever the individual is compresent with is the situation to which he reacts. In the case of behavior, this seems not to be the fact, and we are led to suppose it is only because the mechanical point of view obscures the problem that the facts of behavior themselves define. Supreme interest in the uniformities of behavior has led the behaviorist to overlook important variations in the behavior of different individuals under what has been described as the same conditions. In a perceptual situation, is it accurate to say that an individual's behavior is a function of the total conditions, or must we not say rather that it is a function of certain of the factors that make up the total conditions? What seems to be true is that a situation develops within the perceptual complex, and that it is to this that the behavior is an adjustment. The behavior points to or means that situation.

Now when Weiss supposes that both infant and nurse respond to the clock that is an element of the total electron-proton aggregate he calls the room, he practically admits the distinction I have pointed out, but takes no account of the problem the admission raises. Indeed, he seems to be so blind to it that he says that "the infant if it responds to the clock at all

[8] *Op. cit.*, 40.

will do so by cooing, crying, listening posture, etc.," although the nurse responds by preparing the infant's bath. Now with the possible exception of "listening posture" the list of specified responses of the infant does not describe anything that could be called 'behavior' in a clock-individual situation. I am entirely in agreement with Weiss when he says: "Unless the infant responds to the clock in some specific manner, there is no way for the behaviorist (or for anyone else?) to determine whether the electron-proton aggregate known (to the behaviorist) as a clock, is modifying that aggregate known as the infant." But what I contend is that "cooing "and "crying" are not "specific" responses, and that from these it is quite impossible even to say that the clock-individual situation exists for the infant in question. Is not the contrary position based on the supposition that because the infant's senses are "exposed" to the auditory and visual stimuli of the clock its behavior is therefore determined by them? In principle, this can only mean that the individual necessarily responds *in terms of behavior* to whatever electron-proton aggregate he is at a given time exposed. This surely is not the case. What, therefore, can be inferred from the instance before us is that because the infant's responses lack the character of meaning, because they do not specifically point to or mean the clock, they do not fall under the category of behavior. It would seem then that the criterion of meaning, either explicitly or implied, is necessary if we are able to determine what does and what does not belong to the subject matter of behavioristic psychology.

III. Teleology in Behavior

The correlated group of receptor-neuron-effector elements which underlie the responses of physiological organisms to the physico-chemical conditions of the environment mark off, according to Weiss, a distinct class of behavior, in which the organism adjusts itself to certain effective aspects of the material world. It includes all "the sensory-cerebromotor structures and processes." These structures and processes "represent, with certain extensions, the so-called 'muscle-twitch' phase of human behavior."[9] They are called the biophysical responses.

In view of these definitions, it is perhaps safe to say that the facts that have been chiefly emphasised in the two preceding Sections are of the kind that are here called biophysical. I believe that I am not mistaken in thinking that behaviorism would regard this class of responses as pre-eminently subject throughout to a mechanical explanation, and Weiss does refer to behavior of this sort as presenting "no special difficulties for behaviorism."[10] It is, he says, all of the 'muscle-twitch' variety. Three remarks will serve to bring our discussion, and the facts upon which the discussion has proceeded, into line with these contentions. I have granted,

[9] *Op. cit.*, 43, 120. [10] *Op. cit.*, 120.

in the first place, that facts of a mechanistic sort are involved in behavior, and that it is possible to study these without having recourse to consciousness, mind, or any other term of a non-physical denotation. How much or how little behavior is of this type is not necessary to inquire. We may take, for our present purpose, a view similar to the one that Bertrand Russell[11] develops, namely, that in the field of sensation at least there is nothing that is not an actual constituent of the world with which physics deals, and this may be delivered over bodily to be dealt with in a mechanically causal manner. But, in the second place, the matter is not so simple when we come to perception, even when this is regarded not as a form of knowledge, but as a form of behavior. I have pointed out that observation of behavior of this sort leaves us very far from certain as to what will occur in a given situation, and we are by no means in a favorable position for saying what exactly the causally effective factors are in such a situation. This statement of the case does not rest upon the limitations of present knowledge. The contention is that perception is only a simple case of what may reasonably be expected to occur over and over again in an unfinished and evolving world; that conditions will arise that are novel; and that if behavior takes place it will do so not by the repetition of what is old, but by the invention of what is new. I do not press the contention. I introduce it in order to say, thirdly, that when Weiss reduces perception to the "muscle-twitch variety" of behavior, the biophysical category is stretched to the breaking point, and that, so far from causing the behaviorist "no special difficulties," perception is likely to be as perplexing a problem to him as it has been to the mind psychologist. I have questioned if an adequate analysis of perceptual behavior has been made by behaviorism, and the conclusion seems to be warranted that an element has been omitted that, for the purpose of reference, I have called 'meaning.'

In order to go beyond the point at which we have arrived, it is necessary to look at the other category in which behavior is classified. Here, it would seem, are resumed all responses that have freed themselves from the anatomo-physiological conditions that are the basis of classification for biophysical phenomena.[12] It is the field of overt behavior, and comprises only those motor effects that are conditioned by the individual's responsiveness to the social organization of which he is a member. It is a wide category and includes activities that are usually regarded as belonging to "the educational, vocational, administrative, recreational, and personal forms of behavior."[13] It may with accuracy be said that the biosocial responses are intended to include all, and all the various kinds of, behavior that constitute the special subject-matter of social psychology. Weiss skillfully avoids the question between the leading schools of social psychologists, and between them and the sociologists, as to what constitutes 'social' behavior, but he does so at the cost of giving us merely a negative statement of what he means by "biosocial equivalence." It is, he says, positively not a classification of behavior on the basis of the receptor-effector mechanisms involved. This, he had already maintained, is the characteristic of biophysical classification. In this field, different

[11] *The Analysis of Mind*, 1921, 25, 37 f.
[12] *Op. cit.*, 43.
[13] *Op. cit.*, 136, 137.

sensori-motor conditions give rise to different kinds of reaction. In contrast to this, in the biosocial field, it is the behavior of individual organisms that act as "*stimuli for other individuals*," and, as a consequence, there is said to be "a complete change in the movement groupings" in biosocial responses.[14]

There is, Weiss contends, an impermanence to change in the sensori-cerebro-motor system that underlies biophysical responses, and this may be reasonably interpreted to mean that the neurological conditions of this type of behavior have in the course of ages become relatively fixed. The stability of the nervous mechanisms can be traced for five thousand years. It is too big a question for incidental discussion, and we need a great deal more experimental evidence on the debated question of the inheritance of acquired characteristics before any opinion, for or against Weiss' view, could be said to have any weight.[15] I mention it here to say that a sharp distinction is made between the standardised responses that rest upon such a mechanism, and those that constitute the group of biosocial responses. For instance, there are many *different* biophysical changes that lead to the *same* biosocial response, and, in view of such a fact, Weiss is precluded from finding in the former a principle for explaining the latter. "The classifications which include responses that are biosocially similar need not show any biophysical similarity," he positively asserts.[16] In taking this position, he is well aware, that he is running counter to the large number of psychologists of different schools that look for advance in neurological science to throw light upon all human behavior. He would substitute for nerve physiology the investigation of "the compound-multicellular form of electron-proton organization." When we ask, however, what the denotation of this description is, we discover that it is the behaviorist's way of saying "social." But not to press the point, one does begin to wonder, in the light of the recognized discontinuity between biophysical and biosocial behavior that is positively affirmed in Weiss' denial that these two classes of phenomena are different aspects of sensori-motor organization, what mechanical explanation of the biosocial can be given that escapes the charge of being essentially teleological.

To get this question concretely before us, let us consider the illustration of an invitation to dinner from a friend in a distant city.[17] We may begin by presuming there is involved in this,

[14] *Op. cit.*, p. 43. Weiss' italics.

[15] The position taken by Weiss seems to me one of those temporary and precarious bridges that behaviorists are in the habit of building in their campaign of carrying the gospel of behaviorism, in its extreme form, to the unenlightened peoples who dwell on the other side of the Tiber. I suppose I refer to something in behaviorism that Miss Edgell in *Theories of Memory* speaks of as "camouflage."

[16] *Op. cit.*, 44. [17] *Op. cit.*, 43.

as in all true questions, an element of indeterminism. In a question-situation factors are involved that make uncertain the response, and it is the recognition of this state of affairs that determines the interrogative form the situation assumes. If this were not so, such a situation would never arise; we should never get beyond the categorical, or—which perhaps is as near the categorical as the limitations of human faculty enable us to come—the hypothetical. But, it may be pointed out, it is the existence of true alternatives (disjunction) that gives weight to the illustration, and enables Weiss to argue the independence of biosocial responses of the sensori-motor conditions that form the basis of biophysical reactions. I shall, therefore, assume that there is no objection to regarding the particular techniques of the two categories as distinct, and that they are distinct because an analysis of the behaviors that belong to each shows in the one case, and do not show in the other, an element of indeterminism.[18] What explanation does Weiss offer for this state of affairs? With a good deal that he says in this connection I am in sympathy, and I think he is right in laying emphasis upon the uniqueness of the situation in which behavior, defined so as to distinguish it from, let us say, the impact of one billiard ball on another, is the cause for responses that in its absence would not take place. Behavior is a stimulus for other individuals, and the medium in which such stimuli are effective is the social group. When, therefore, it is said that it operates as a cause, I take it that we are to understand by behavior the *characteristic* responses of biological organisms, in distinction from the changes in molecular state that act as causes in the physical world. Stated otherwise, Weiss emphasises the importance of distinguishing between the region in which the equations that express our knowledge are reversible, and that in which our equations, in the nature of the case, never can be. What other distinction can be meant if the biophysical stands in such a decided contrast to the biosocial, as he says? And upon what factor can the finger be laid, in the latter case, that is the bearer of this indeterminism? If Weiss' friend in a distant city does not know if a dinner invitation can or cannot be accepted, must there not be something in the situation of that electron-proton aggregate that Weiss calls his friend upon which the answer finally rests? And whatever this factor may be, it is excluded, on the view we are examining, from the realm of mechanical causation. The conclusions then to which we come are

[18] I have shown in a previous Section that I would draw the line that separates the sphere of determinism and that of indeterminism at another place, and much lower down in the phylogenetic history of the species than does Weiss. That however is an unimportant detail. What matters is the recognition of the distinction.

these: (a) that behaving organisms, as behaving, affect one another in a way that differentiates them from non-behaving things; and (b) that the principle of determinism that suffices for the explanation of the latter does not apply to the former.

Now this is exactly the state of affairs we should expect to find if teleology were a characteristic of behavior. In order to keep our point of view as empirical as possible, it may be affirmed that teleology is as much a matter of fact as mechanism is. In both cases, we want to know if behavior has or has not certain specified characteristics. Ultimately, teleology rests upon an affirmative answer to the question: Does the behaving individual contribute factors to the conditions under which behavior takes place so that, if all the other factors remained the same, the behavior would, in this case, be different?

I have pointed out that this set of conditions is realised in genuine questions. It is also realised in human wishes. For their fulfillment, wishes depend in part upon the individual who entertains them, and in part upon other individuals for whom they are not wishes but, what Baldwin[19] calls, 'proposals.' What for me, therefore, is psychologically a want is for you a course of action. Something like this, in simpler or more complex form, is the fundamental structure of most, if not all, social situations. Whether I realise my wish,—whether in any social relation the wish of anyone is realised,—depends, in the final count, upon the particular contribution that is made by some individual or individuals other than the one who entertains the wish. Wishes are of varying degrees of complexity, and they may last for a few seconds or endure for days or years. They may affect the structure of the society within which they arise not at all; or they may, when realised, involve its complete reorganization. These are merely incidental variations of the class of phenomena we are considering, and we have only to mention them to see how large and important a place they play in human life. They belong to the group of biosocial facts. Interpreted in behavioristic terminology, wishes are, or are expressed in, some form of behavior—speech reaction, if you please; they act as stimuli that release, or do not release, in other individuals responses upon which the realisation of the wishes in question depends. If I am correct in supposing that we have here an example that embodies factors that are present in all or most social relations, it ought not to be difficult to agree that indeterminism is a characteristic of a very large part of human life. And further it ought not to be difficult to agree that such indeterminism as exists has its basis in the non-mechanical nature of the laws that are expressed in social behavior.

[19] *Thought and Things*, 2, 151 f.

If I have understood his position, Weiss, I anticipate, will agree that the foregoing statements and deductions are substantially correct. In that case, I should like to point out that wishes have to be interpreted, from the behavioristic point of view, as part of the conditions under which behavior takes place. They create a situation in which it is possible for acts to occur that, apart from those conditions, would not take place. But is not this exactly what the word teleology has in the main stood for in all its long history? There are no doubt reasons why the scientist regards the term with disfavor, and his nervousness apparently is not overcome by any substitute that may be offered. This is particularly noticeable among the nominalists, with whom words function too often, in our own and other centuries, as unrecognised essences. But be that as it may, and let our terminology be what it will, there is urgent need to be called to a frank recognition of facts, and behaviorists least of all should hesitate to respond to the empirical call.

I have referred to the existence of objections to the word teleology on the part of scientists, and so far as these objections are based on the fact that particular interpretations of the term obscure our insight into the nature of the physical and organic worlds, they seem to me valid. Suspicion of the word and what the word stands for took the form among psychologists of an attempt to write a psychology "without a soul," and the same attitude is seen in the criticisms that were directed against the notion that the mind was made up of a number of separate faculties. For what, it was asked, is the 'soul' but the survival within the field of psychology of the *deus ex machina* of theology, and are the so-called 'faculties' anything more than a survival in pluralistic form of the same conception? In neither case is there a description of observed facts, and used as an explanatory principle the 'soul' or the 'faculties' tempts one to indulge poetic fancy where regard ought to be had for the rigors of scientific proof.

Notwithstanding all objections and difficulties connected with the notion, teleology has never quite died, and at the present time McDougall has become the spokesman for it as the only means by which psychology can advance to the position of an independent science. In his *Outline of Psychology*, and also in his Presidential Address before the Psychological Section of the British Association for the Advancement of Science, McDougall has entered a plea for "purposive striving" as a fundamental psychological category. If I understand him correctly, McDougall is led to this position through dissatisfaction with what he regards as the one-sideness of both the behaviorists and the introspectionists.[20] He agrees with the introspectionists in saying that facts of a mental order must be included among, if they are not the whole of, the data of psychology, but, he argues, so long as the introspectionist maintains the structural-analytic standpoint he can never achieve an explanation of the

[20] *Op. cit.*, 1923, ch. 1.

facts with which he deals. His disagreement with the behaviorists is of a more radical sort. For he contends that when they bring over into the field of psychology the methods of description and explanation that have been developed in the physical sciences, they not only subordinate psychology to an outside control, but render themselves incapable of observing the subject-matter that has historically belonged to psychology. The situation is no better among those who try to combine the analytical description of consciousness with the mechanical explanation of the behaviorists, for they are bound to have recourse either to psycho-physical parallelism or to epiphenomenalism, and, in either case, this results in nothing but "a lame excuse for a science." It is time, he says, that psychologists took themselves seriously as scientists, and accepted the task of developing psychology as an independent discipline. And this can be done, he contends, by adopting without reserve "the conception of purposive striving as valid, useful, nay, indispensible and therefore true."

Above I called attention to Weiss' objection to mind on the ground that it involved a definition of the type *"that something which* specifically may be characterized as redness, etc." This he argues is tautology, because there is nothing in addition to the redness, sweetness, etc., nothing, that is to say, except these definite experiences, that remains to give meaning to the word mind. The mind isn't red or sweet; red or sweet, as concrete data of experience, are what must be meant by mind, if anything is meant. Now it is interesting that McDougall adopts this type of definition of mind in his *Outlines*, and insists that the mechanistic psychologist cannot escape it. "If," he argues, "we refuse to follow this modern practice (of substituting the nervous system for mind), we must go back to, or adhere to, the older practice and still speak of the mind. For what the older psychologist meant by "the mind" was that *something* which expresses its nature, powers, and functions in two ways: (1) the modes of individual experience; (2) the modes of bodily activity, the sum of which constitutes the behavior of the individual. The mechanistic psychology says—this *something* which expresses its nature in these two ways is essentially the brain, as described to us by the mechanistic psychology. There can be no doubt that we are bound to postulate this *something*, and that, if we are not content to regard it as merely the brain mechanically conceived, we must have a name for it. . . . I do not think we can find a better word to denote this *something* than the old fashioned word 'mind.' The mind, then, of the individual organism is that which expresses itself in his experience and in his behavior."[21]

It does not fall within the scope of this paper to examine McDougall's views in detail, but no consideration of teleology would, at the present time, be justified which omitted all reference to the position he has taken on this subject. The most that I shall attempt is to state two questions that are often confused, and are really distinct, and to indicate an attitude of mind in which, it seems to me, the questions may be approached.

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[21] *Op. cit.*, 35.

The first question is this: Is "purposive striving" as a fundamental psychological category necessarily connected with a substantial conception of mind? That McDougall does take such a view of mind is evident from his statement of position I have just quoted. From one point of view, McDougall's assumption of the Cartesian position places him in no worse case than, for example, Weiss is placed in assuming physical monism. But in neither case do we squarely meet the ultimate question. For it is one thing to say that a philosophical view may be adopted for its methodological serviceableness in science, and it is another to say that the content of a science is a justification of a particular philosophical view. Now the question I have raised is more closely connected with the assumption of the latter point of view than with the former. It would clear the air if we were told whether the acceptance by the psychologist of "purposive striving" committed him to the substantive view of mind. Historically, I think, there is no unequivocal justification for this contention. But that some such connection as I have indicated is implied in McDougall's statements seems to be the general impression. I feel that this is unfortunate, because it places an unnecessary obstacle to the unprejudiced consideration of McDougall's psychology.

Let us return to the quotation above. Frankly, I may say that I am not satisfied with this way of stating the case. My dissatisfaction is based upon the feeling that it confuses, or seems to confuse, logical with psychological analysis. We may, of course, say that both experience and bodily activities *imply* a subject which acts in certain observed ways; but it is doubtful if this subject ever comes within the range of the methods by which we become acquainted with the modes of experience and bodily activities that are said to manifest its "nature, powers, and functions." This objection raises in a particular form the difficulty that was expressed historically with respect to the Cartesian *cogito ergo sum*. The *sum* it was held must be either an inference or an implication of the *cogito*. In the former case, it is not a datum of experience; in the latter, it is only an indirect object of observation. For what we directly observe are sensa,—colors, sounds, tastes; and, if the question arose as to what sensa reside in, the answer would be in material objects, not in a subject of experience. The relation of the subject to its experience, however, is not a scientific question, and we can hardly think it possible that McDougall is calling upon his colleagues to take the philosophical way to the founding of an independent psychology. We can hardly think so because this course would be open to the same kind of objection he has himself brought against the behaviorists' attempt to make psychology subordinate to physics and chemistry. If, therefore,

this is not what McDougall intends, there does not seem to be any particular advantage in the way in which he states his position. It would surely be enough, to start with, to say that by "mind" he means the "modes of experience and the modes of bodily activity," and that the word "behavior" is to be understood as a synonym of "mind". This is, indeed, what the passage practically amounts to when it is shorn of its Cartesian implications; and, in this form, it would seem to offer a more favorable point of departure, and to provide a broader basis of cooperation.

If this is a satisfactory solution of the difficulty implied in the first question, the ground is cleared for a consideration of the empirical evidence he advances in support of "purposive striving." The second question, therefore, to which attention should be given is this: What account, other than a philosophical one, does McDougall give of the category of "purposive striving?" What are the empirically observable facts upon which a justification of the category is shown to rest? This is a question that cannot fail to receive considerable attention, and, since this paper was written, Weiss[22] has given it partial consideration. I have done all that is possible within the scope of my purpose in this paper to indicate how teleology may be approached from an empirical point of view, and it is perhaps some service to have indicated how McDougall's position may be brought into the focus of current discussion.

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[22] *Psych. Rev.*, 32, 171 f.

THE PSYCHOLOGY OF *GESTALT*[1]

By HARRY HELSON, University of Illinois

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The general reader is advised to omit the small print in the following two Sections, which deal with technical material of importance principally to the specialist. The remaining Sections show how configurational principles apply to hearing, touch, simple and complex behavior patterns, memory, learning and the higher thought processes, and comprise a brief discussion of the psychology of the abnormal and the general implications of configurationism. Since Koffka's and Köhler's books have appeared in English, I have decided that more than passing reference to them is unnecessary, and that a more detailed consideration of experimental sources (which will probably never be translated into English) will be more useful and enlightening in tracing the developments and implications of *Gestalttheorie*. The nature of the original material precludes the possibility of any such systematic presentation of topics as I gave in the instance of movement, but I have tried to draw a coherent and clear picture of the important subjects touched upon by the configurationists in the various fields of experimental psychology.

(16) *The Effect of Configurations upon After-images*

The configurational effects upon after-images have opened up some interesting problems. It might readily be granted that the configuration of a physical structure will decide what is given in perception. It might also be taken for granted that, if the objective configuration is equivocal or weak, imaginal factors may play a part in determining what is perceived. After-images seem to fall between experiences which are wholly sub-

[1]This is the third article in a series on the psychology of the *Gestalt*, the first two of which appeared in this JOURNAL, 36, 1925, 342-370, 494-526.

jective and those which are determined by stimuli; in other words, they are too nearly objective to vary directly with purely internal changes and too nearly subjective, or too far removed from stimulus, to be universally determined throughout their temporal course by stimulus.

Experiments were carried on by Frank in which after-images were projected on screens on which were drawings of three-dimensional figures,[2] figures in perspective, figures which represented figure-and-ground effects, letters with contours and the like. Frank found that after-images tend to accommodate themselves to the form and size of the drawings of three-dimensional bodies, just as they do when projected on real bodies in space. An after-image thrown on a drawing of a corridor appeared small when it was localized at the near end and larger when localized at the far end of the corridor. If the after-image is thrown on a drawing whose fields can be seen as figure and ground, it will be clear (*deutlich, klar*) when it is on the figure and blurred when projected on the ground, partaking in this case of the properties of each field as it forms part of each. If the fields are reversed, so that what was figure becomes ground and conversely, the after-image which was blurred on the field as ground becomes clear on it as figure.

Fuchs and Katz[3] (the latter not a configurationist) found that, when an after-image was projected on an object so that it protruded beyond the outline of the object, the part covering the object was transparent and remained so as long as the contour of the after-image was preserved. The influence of the configuration upon the after-image was shown in a positive way by Fuchs by means of the following procedure: a stimulus was presented which was composed of differently colored parts, forming two discernible configurations, each with its own color. If one figure was seen, it had a certain dominating color throughout, but if the other figure was seen, another color predominated. A circle in the center of these figures contained a mixture of both colours. If the *O* saw one configuration, the after-image of the middle circle took on the color of the dominating configuration, no matter which configuration was preferred in perception. If both figures were perceived as a single configuration, it had a uniform color in the after-image as well as in direct perception.[4]

Fuchs, contrary to the usual theories of after-images, believes that after-images may be influenced as much by central processes as by peripheral.[5] In fact, Fuchs argues for the central origin of colors as well as for the central origin of configurations and after-images, in view of the fact that, as in the example just given, the color may vary with the after-image. The central theory of after-images receives support from certain observations made with abnormal *Os*. Pathological *Os*, suffering from hemianopsia, were able to see complete figures when part of the stimulus-configuration fell within the blind portion of the retina, and were also able to obtain complete after-images of these configurations. Often, when per-

[2]H. Frank, Ueber die Beeinflussung von Nachbildern durch die Gestalteigenschaften der Projektionsfläche, *Psych. Forsch.*, 4, 1923, 33-42.

[3]W. Fuchs, Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung, *Z. f. Psych.*, 91, 1922, 201 ff. Katz, Die Erscheinungsweisen der Farben. *Z. f. Psych.*, Ergänz. Bd., 7, 1911.

[4]W. Fuchs, Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten ("Angleichungserscheinungen"), *Z. f. Psych.*, 92, 1923, 288 ff.

[5]W. Fuchs, Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter; Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker, *Z. f. Psych.*, 83, 1920, 67-169.

ception failed to reveal a complete figure, the after-image would contain more of the figure than the original. Finally, when a part-circle was seen to be incomplete, a complete circle was seen in the after-image. When the circles were composed of different colors, one half being colored in one color, one half in another color, sometimes the entire circle was seen in one color, sometimes in two. But the completion of the figures takes place only when there is sufficient stimulation of the normal portion of the retina for the formation of a configuration. Fuchs' preference for central explanations, in place of explanations by means of peripheral mechanisms, is illustrative of the general tendency of the configurationists to push explanations, whenever possible, back into the central nervous system.

Many of the phenomena noted by Fuchs were observed by Rothschild in his study of the influence of the configuration upon negative after-images.[6] Rothschild presented figures made of white woolen threads on a black background. He used simple unitary curves and line figures, and was able to reproduce them with no change in form. Little irregularities in the originals were often corrected in the after-images, which showed a tendency to become "good" configurations. But line figures which were not composed of a unitary line (*Linienverlauf*) often gave different results; the whole figure would not appear at once, parts would appear in rivalry as simple unitary figures in themselves. Thus an arrow gave first the tail end, and then the head, in alternation. That these and similar phenomena were not due to position in the visual field or to attention was proved by the fact that it was possible to perceive a single, good configuration when the parts extended far into the periphery of the field of vision. Attention to a part may (as Fuchs found) cause the configuration to disappear.

Outline figures which were incomplete were completed in the after-image. This rule does not hold, however, for surface figures where other conditioning factors are too strong to allow completion to take place. Complex figures often gave alternation of different part-figures, especially when the parts themselves formed independent figures or when the original did not possess a definite, universal structure.

(17) *The Influence of Form upon Color*

Does the configuration in which a given quality appears exercise any effect upon that quality? Or are qualities unique phenomena, independent of their position within a structure? Are qualities amenable to configurational laws? The configurationists have sought to answer these and similar questions as they applied the concept of *Gestalt* to traditional problems of color, psychophysical limens and contrast. These problems concern fundamental points of view, and in dealing with them we shall have occasion to note wherein the configurationists have adopted a new attitude toward many of the old facts.

Fuchs'[7] experiments on the influence of form on the perception of color are guided by such questions as these. Does the critical part of a configuration influence the parts around it? Must the parts be in immediate contact in order to be influenced by the rest? Do assimilative effects appear in after-image and contrast colors? The first question was answered by observing an orange E exposed through a 30° opening of a blue episco-

[6] H. Rothschild, Ueber den Einfluss der Gestalt auf das negative Nachbild ruhender visueller Figuren, *Arch. f. Ophthalmol.*, 112, 1923, 1-128.

[7] See reference in Note 4.

tister. The E will appear either (1) in two parts, of which one part is more blue or gray than the other while the second part is orange, or (2) as a complete letter whose color is orange while the part surrounding it is a shaded orange. The experiments showed that even though certain parts may differ from the rest in color; the greater part will influence the rest; the color of the larger part determines the color, of the whole. But the color of the larger part is itself more or less changed. If any part of the figure is attended to, it keeps its own color, it is necessary for the configuration as a whole to be perceived if it is to assume a uniform color. These facts show, says Fuchs, that a configuration is a unitary surface consisting only of a single piece.

Even though the smaller part is different in color from the rest it may possess greater intensity or impressiveness, and it will thus compensate or overcompensate for the predominance of the color of the larger part. Under these conditions the color of the whole will be a mixture of the two or will even lie wholly in the direction of the color of the smaller part. The color of the smaller part may exert a greater influence over the whole than its size warrants, if it forms the crucial part (*Schwerpunkt*) of the configuration. If a configuration has several important points of reference, the crucial point may be shifted from one to another. Thus the letter E has at least two crucial parts, the lower horizontal line and the vertical line. Some interesting facts emerge when the E is broken up in different ways. We may summarize these facts as follows: the color of the other parts influences to some extent the color of the critical part; the color of the critical part deviates more or less by assimilation of the color of the other parts; the mutual assimilation-effects of the parts are so much the greater, the more or the better the perception of the total configuration. The limiting case would be that in which every surface should be of the same color as each of its parts. Assimilation would therefore arise as a result of configurational perception, not as the result of the assimilative tendency of neighboring retinal fields by diffusion.[8]

Under proper conditions, a configuration may arise from separate elements. If five yellow circles, separated by a small interval and forming the outline of a square with a spot in the center, are observed and if a shadow has been thrown on the middle circle, it will appear like the rest in color, sometimes as shaded and at other times not. If the first, second and fourth are perceived as a square, then the middle one (the third) appears darker than the rest. If the first and second are in a single configuration and the third is made a part of it, then even though it is a darker shade of yellow than the first two it will appear like them in color, but as opposed to the fourth and fifth the first three appear to be a grayish yellow.

Fuchs found that geometrically simple figures need not be psychologically simple. The triangle is a figure in question; although it is geometrically simple it is difficult to get configurational effects from it psychologically. Factors of simplicity have yet, Fuchs admits, to be investigated more fully. The square and the circle are geometrical figures which lend themselves to configurational effects very easily, as we found above. Some interesting effects were found in the following arrangement: nine circles were arranged to form a square, the four outer being of a blue-green color, the four inner being yellow, and the center one a mixture of the other colors. It is here possible to see simultaneously a blue-green square and a yellow cross within it. In this case the middle circle is yellow. If one sees a blue-green cross, tilted toward the left, and a yellow square, also tilted, the middle circle is blue-green. If one perceives all nine circles as a single configuration, they take on the same color, which will be either yellow or

[8]Fuchs, *op. cit.* (Note 4), 264-265. For Exner's theory of retinal diffusion see *Pflüger's Arch.*, 73, 1898, 177 ff.

gray. The same thing holds true of the after-image; here the middle circle will take on the color of the figure to which it belongs. In this manner the configuration determines the quality of its parts.

Assimilative effects within configurations were also observed. Different kinds of assimilation may take place; assimilation may be between a contrast color and the color of its surroundings. An example of the first type is found in the following simple experiment: if two circles cut out of the same gray are laid respectively on a white and a black paper and the whole is covered with a piece of gauze paper, the two circles are different in color if perceived independently; but if both are perceived in a unitary structure their difference in color vanishes, apparently because mutual assimilation has taken place between them. Wundt's explanation of this phenomenon rested on the assumption that the original contrast phenomenon is compensated by a mutual associative effect.[9] If Fuchs' explanation is correct, associational factors are not in question; "nach unseren Ergebnissen kommt aber eine Assoziation nicht in Frage, sondern: unter der Wirkung der Gesamtgestalt der aus beiden Kreuzen samt Verbindungsbalken sich ergebenden Figur tritt positive Farbänderung, d. h., Aenderung der Wahrnehmungsinhalte ein."[10]

Assimilation between the contrast colors and colors of the surroundings took place between black squares interstiched with white narrow lines. The squares and lines tend to assimilate to the surrounding colors as either one figure or another is grasped; if the white lines are fixated, so that they protrude from the rest, gray spots will appear on them due to the surrounding black squares. Color assimilation takes place best when the configurations possess a strong coherence-grade and the colors merge gradually into one another. Experiments in contrast and assimilative phenomena show that with the same stimuli it is possible to get assimilative or contrast effects by changing the perception of the configuration.

Benary, Gelb and Granit have found that contrast effects are different according as the contrasting parts belong to the figure or to the ground. It may be said that the work of these men consists in getting a closer approximation to the factors influencing the quality *within* the configuration itself.

Benary places two gray patches on a black cross in such fashion that the one is within the black field of the cross and the other forms part of its ground.[11] The latter actually is surrounded by more black than the former and should therefore, according to summative theories, appear lighter than the other by contrast. But the gray in the cross actually appears lighter. Brightness therefore depends upon whether the field to be tested forms part of the figure or ground, whether it is part of a configuration or not. But there are other factors working against the figural effect; so that the gray forming a part of the figure is not always the lighter. The configurational factors may work against one another in so many different ways that it is at present impossible to draw definite conclusions from the results of all the work so far reported.

(18) *Auditory Configurations*

Auditory phenomena group themselves very readily into temporal and non-temporal configurations. A single tone or chord, perceived almost instantly, represents a non-temporal configuration. But themes, melodies and the complex patterns

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[9]W. Wundt, *Physiol. Psychol.*, 2, 1902, 261 ff.

[10]Fuchs, *op. cit.*, 298.

[11]W. Benary, Beobachtungen zu einem Experiment über Helligkeitskontrast, *Psych. Forsch.*, 5, 1924, 131 ff.

of sound and harmony that we hear in a choral or symphony require time for their completion, and hence may be called temporal configurations. The presence of specific form-qualities in tonal complexes was noted by Mach, von Ehrenfels and Meinong. Mach saw in the new qualities peculiar space-forms and time-forms which were merely new sensations. Meinong ascribed the properties of a melody not compoundable from its parts to a special founding process and the transformations which such a process works upon sensory contents.[12] The configurationists, rejecting the space-forms and time-forms of Mach and the "activities" of the Graz school, assert that temporal configurations simply require a time-parameter, and the changes in time which it permits, to account for the configurational properties of the completed structure. Even though dynamic processes in time are difficult to explain, they are subject to configurational laws and satisfy the criteria of true structural events.

The problems of sound, from the configurational point of view, become almost endlessly complicated by the multiplication of facts of "higher order." Thus the problem of the resolution of qualitative patterns, whether in a single tone or a chord, difficult as it may be, is but the first of a number of even more vexing problems concerning the nature and conditions of the more complex auditory phenomena such as fusion, combinational tones, localization, distance, monotic and diotic hearing. If we also take into consideration the phenomena described under the head of "space induction" in sound, and the relations of tones and noises, then it seems an almost hopeless task, from the point of view of any psychology, to bring all the facts under a single ordering principle. Since a systematic investigation of all these problems remains to be made, I can touch upon the bearing of configurational theories on the field of audition only in so far as experimentation has yielded definite results in the most important problems.

Beginning with the simplest auditory configurations, a single tone or clang, we may describe it as a complex unity in which any modification in intensity, pitch, volume, brightness or vocality will affect the whole. According to older assumptions, the perception of unitary clangs or chords possessing super-summative properties was purely phenomenological (*Schein*), and could be dissolved into simple components as soon as the effects of analytic attention revealed the multiple nature of the experience. But granted that it is possible to analyze partial tones and this or that attribute from a tonal complex, the configurationists insist that phenomenal structures possess unique properties paralleled in the nervous system and lost in analysis. The reality of total clang as such cannot be questioned in

[12] *Cf.* M. Bentley, Psychology of Mental Arrangement, this JOURNAL, 13, 1902, 268-293 for the school of the *Gestaltqualität*, and the first article by this writer, *op. cit.*, 343-344.

the light of such experimental evidence as Eberhardt offers: a partial too weak to be heard may still influence the total clang color, while every partial tone in a total clang suffers in intensity as a result of its membership in the configuration,—not because of attention, weak or uncertain "apperception," or the like, but because of configurational principles applicable to the physical and physiological structures as well as to the phenomenal. Certain discrepancies between physical and phenomenal configurations or their components still remain, but these can be removed only by a systematic variation of subjective and objective conditions.[13]

Passing to some of the more complex aspects of hearing, we find essentially the same arguments advanced by the configurationists against summative theories of the physiology and phenomenology of monotic and diotic hearing.[14] Unitary, diotic hearing does not consist of a summation of two monotic processes; it does not build itself up out of monotic processes or parts of them. The primary processes do not unite into secondary processes or enter into them as fundaments. The processes in each ear furnish only the necessary and sufficient conditions for the unitary structures found in diotic hearing, just as the inaudible primary tones are necessary for the perception of the beating tone. Some experimental results support this statement: von Hornbostel found that beats could be produced in each ear independently of the other, and that over and above the dichotic beats could be heard beats of a higher order. This phenomenon cannot be a result of central *Ueberlagerung* of right and left processes. Nor is there a summation of intensities; for when both tones are definitely below the threshold, double hearing does not bring them above it.

The configurational theory of hearing advanced by von Hornbostel is designed to account for the phenomenology of hearing by preserving characteristic properties of total structures. Any theory of hearing, he tells us, must proceed on the assumption that double (diotic) hearing is the original form of hearing from which monotic hearing is derived. Diotic hearing cannot be derived from monotic; localization, direction and other spatial attributes of sound are largely functions of diotic hearing. Allers and Schmiedek object to this theory on the ground that monotic hearing possesses spatial attributes *per se* which are of importance for diotic hearing, just as monocular moments are conserved in binocular vision.[15] But von Hornbostel's theory is characteristically configurational in that he would explain the facts of single hearing and discrete stimuli by the unitary central processes, going, as Wertheimer would say, *von oben nach unten.*

The auditory perception of space, whether by means of monotic or of diotic hearing, becomes tremendously complicated when we consider the rôles played by vision and kinaesthesis in sound localization. Indeed, some writers assert that audition possesses no spatial characters like direction, distance, and position, except in so far as visual and kinaesthetic images are associated with the primary auditory data. The primary difficulty with associative theories in this field, as Allers and Schmiedek point out, is fatal. How can any datum acquire spatial properties if spatiality is not somehow resident in it? On the other hand, the facts described by Klemm and embodied in the following law of spatial induction or complication challenge all theories: simultaneous, disparate stimuli (*e.g.*, vision, sound and pressure) exercise an inductive effect upon each other; this effect can be characterized as a tendency toward spatial fusion. The transition from

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[13]M. Eberhardt, Ueber die phänomenale Höhe und Stärke von Teiltönen, *Psych. Forsch.*, 2, 1922, 346ff.

[14]The discussion here follows E. M. von Hornbostel, Beobachtungen über ein- und zweiohriges Hören, *Psych. Forsch.*, 4, 1923, 64-115.

[15]R. Allers and O. Schmiedek, Ueber die Wahrnehmung der Schallrichtung, *Psych. Forsch.*, 6, 1925, 92ff.

one sense modality to another and the unitary effects arising from the fusion of sense data from different modalities are inexplicable on any atomistic-associationistic theories. Some common materials and processes must be at the basis of such phenomena. The configurationists have attempted to frame a set of hypotheses to account for the common denominator in the various sense modalities; but as yet the field is an open one, and challenges investigation and new ideas.

The figure-ground properties of visual fields find an analogy in hearing. Sounds arise from a background of stillness or a diffuse acoustic plenum. The sharp outline, solidity, and definite localization of diotic sounds correspond to the figure. Monotic sounds are less objective, more unstable and less predominating than diotic. The effectiveness of a sound is greater when heard with two ears than when heard with one, even though both are of the same intensity.[16] Diotic hearing is the result of the formation of strong configuration, in Köhler's sense.[17] The transition from strong to weak configurations follows this general law of hearing: diotic hearing changes into monotic when the stimulus becomes in any respect less unified. This change must be due to a change in central physiological processes which have, in time, become weak configurations. The strongest central configurations arise from a stimulus in the median plane, and are the more effective, the higher the frequency of the stimulus, the more pronounced its wave-form, the louder it sounds, and the shorter time it lasts. Monotic hearing, on the other hand, mediates the perception of distance in sound; for the nearer a sound seems to be (with certain exceptions), the more closely it approximates to monotic hearing. Attributes of distance, like "near" and "far," are as basic phenomenological qualities of sound as the so-called primary qualities, because the soft sound is always judged as far, the loud sound as near, while soft near sounds are confused with harsh, sharp far sounds.

So far we have been dealing only with non-temporal auditory configurations perceived in a specious present. Such phenomena may be called static or stationary, after Köhler, because there is no such experience of a change in time as there is, *e.g.*, in a melody, which is a temporal structure.[18] When, therefore, the components change in time and the time intervals affect the components (*cf. allegro* and *andante* tempos), we say there are dynamic, temporal configurations. The rate of change may exercise a marked effect upon the phenomenal structure: it is

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[16]von Hornbostel, *op. cit.*

[17]According to Köhler, weak configurations do not depend upon a determining topography for their form and have no real space form. The temperature of the bodies in a room (or molecules in a gas) is the same, but a change in the temperature of the one is accompanied by a change in the temperature of the others; the division of warmth is a *Gestalt*, but does not depend on the form of the objects. *Physische Gestalten*, 126.

[18]Köhler distinguishes five different kinds of configurational states, which must be kept in mind during the rest of this paper: (1) static states are represented by bodies in perfect equilibrium with no apparent motion, like a book lying on a table; (2) stationary states preserve a characteristic form though the parts are changing, as in the flow of current at a given point; (3) quasi-stationary states include those which are too slow to reveal any dynamic factors but are really changing; (4) periodically stationary configurations are found in resonating membranes; (5) dynamic configurations can be viewed in change, *e.g.*, the fall of a body in a vacuum. *Physische Gestalten*, Introduction.

said that Charles Darwin was unable to recognize the melody of "God Save the Queen" when it was played very fast. Stimuli may reach each ear at different times and yet be perceived as simultaneous, transformed into stationary structures, provided that the time-interval between them is not too great. Experiences of simultaneity may be said to correspond to Wertheimer's simultaneous stage of apparent movement. If the second stimulus is too late to form a unitary structure with the first, no *Zeitspannung* can take place, and a new process is the outcome. This new process will appear as a sound which has been displaced to one side, as in monotic hearing, from the median plane of diotic hearing. Audition may thus be an instrument for spatial orientation no less than vision.[19]

(19) *Tactile Configurations*

Owing to the large number of varied qualities mediated by the end-organs within and below the skin, the number of tactile patterns must be very large. The possibilities of spatial patterns of warm and cold, pain and pressure, and other qualitative combinations have yet to be investigated. An interesting problem calling for consideration by the configurationists concerns the effect of the various tactile qualities upon each other as they blend in physiological or phenomenal configurations. The phenomena of tactile phi or apparent movement on the skin have been investigated by Benussi and Scholz, and striking similarities have been established between touch and vision.[20] On the other hand, Gelb and Goldstein doubt the existence of a tactile space except as built up through vision and kinaesthesis, so that, if their position proves tenable, a large class of spatial configurations must be denied to this sense modality.[21] Temporal configurations may be found in this domain; for any change in time may be regarded as a dynamic process possessing configurational properties. Indeed, von Ehrenfels mentioned reddening, cooling and blanching as examples of changes characterized by a peculiar form-quality.

The doubtful status of spatial configurations on the skin leaves us only the qualitative patterns to consider. Von Frey

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[19]The argument in the main follows von Hornbostel.

[20]V. Benussi, Versuche zur Analyse taktil erweckter Scheinbewegungen, *etc.*, *Arch. f. d. ges. Psych.*, 36, 1917, 59-136; W. Scholz, Experimentelle Untersuchungen über die phänomenale Grösse von Raumstrecken, die durch Sukzessiv-Darbietung zweier Reize begrenzt werden, *Psych. Forsch.*, 5, 1924, 219. An account of Scholz' work has been given in the second article cited in note 1.

[21]A. Gelb and K. Goldstein, *Psychologische Analysen hirnpathologischer Fälle*, 1920, 157-251. For a brief account see J. Fröbes, *Lehrbuch der experimentellen Psychologie*, 1, third ed., 355ff., 365f.

has pointed out[22] that, if Müller's theory of the specific energies of nerves is true, then sensations mediated by the nerves would be capable only of quantitative changes, while every slight change in the quality would require a different kind of sense organ,—a requirement which has not and is not likely to be met in physiology. So the view has grown that the kind of stimulus, the manner of stimulation, the temporal relations of the stimuli, and their spatial juxtaposition all affect the quality of the resulting experience. Two tactile stimuli applied simultaneously or successively may influence each other in a variety of ways, depending upon their mutual intensities, position on the skin, and the time interval between them. Contrast, fusion and compensatory phenomena are matters of common enough occurrence to illustrate the qualitative and quantitative changes which single "sensations" undergo when several stimuli combine in the nervous system.

What has been taken to be a simple, elementary sensation, like the prick sensation of Goldscheider, is in reality a complex experience, says von Frey, for which as yet no satisfactory analysis into components has been made. Hence disagreements may arise regarding this sensation, since different investigators may have been talking about different aspects of a complex phenomenon. Even the stimulation resulting from the mechanical application of single points on the skin is attended by irradiation, which forces us to regard the resulting complex as a unitary whole and not as a plurality of juxtaposed excitations. Just as the stimuli for apparent movement are separated in space and time, so may tactile stimuli be separated and still be perceived as unitary structures, *e.g.*, tactile phi and the sensation of synthetic wetness.

The difficulty, if not the impossibility, of analysis in the field of touch is due, von Frey believes, to the fact that analysis cannot penetrate into those levels of psychic phenomena which lie below the threshold of clear consciousness (*deutliche Bewusstheit*), perhaps wholly on the physiological level. The integrative action of the nervous system as well as the supposedly electro-chemical nature of its activities implies that the physiological processes at the basis of tactile experiences possess supersummative properties.[23] [24]

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[22]M. von Frey, Ueber Wandlungen der Empfindungen bei formal verschiedener Reizung einer Art von Sinnesnerven, *Psych. Forsch.*, 3, 1923, 209-218.

[23]Köhler, in *Physische Gestalten* (244), feels that tactile space forms may be perceived without the aid of associated visual images. Gelb and Goldstein in the article cited above come perilously near an associationistic theory in trying to account for their abnormal patient who depended mainly upon kinaesthesis for his perceptions of form. If this difference of opinion is taken seriously, it means that much of Köhler's physiological theory must be changed, perhaps some fundamental assumptions in *Gestalttheorie*.

[24]There is little to be said about taste and smell configurations. It may well be doubted if they exist in any well-defined sense. In so far as all processes in time may be regarded as subject to configurational principles, taste and

(20) *The Problem of Relations and the Psychophysical Judgment*[25]

How can dynamic processes extended in time form a unity? How is it possible to judge the relations between two or more stimuli separated in time if there is no *real* basis for the judgment? These and many other questions concerning the temporal configurations call for answer. With them we shall find involved problems of memory, recognition and reproduction. Prevalent theories designed to explain the continuity of experience and behavior rest upon the concepts of association or frequency-linkage, special attentive or unifying acts (simultaneous and successive), memory images and the like. In fact it is not too much to say that every system of psychology built upon atomistic-associationistic assumptions has failed to make a logical transition from the cross to the long-section facts of behavior and experience. The attempt to build complex patterns extended in space and time out of successive phases of simple units illustrates a general tendency on the part of psychologists to cling to a simple logical scheme whose weakness is repeatedly demonstrated when it is brought to the bar of experimental observation and verification. Since the units composing experience or behavior have been present in the past, the associationistic-frequency theories ultimately rest upon the magical effects of "past experience," which can be made to explain anything in any way one likes. Yet, strange to say, the shortcomings of the associationistic theories rarely led to anything more than additional assumptions serving to bolster a simple, but inadequate logic, while the fundamental point of view remained unaltered.

There are really a number of problems calling for solution when we consider the unity of experience and the fundamental facts of behavior as we find them exhibited in processes running their course in time. The simple associationistic theories do not tell us how it is possible to say that a is greater than, equal to, or less than b when a and b are offered either simultaneously or successively. Actual observation proves that images, secondary processes and other introspectable residua may be entirely absent when a judgment is made; hence the concept of past experience does not help unless it is used in a very definite, restricted sense. We can best approach the problems of the temporal configurations by a discussion of the psychophysical judgment and of relations, since these are basic in the transition from

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smell would have to be included. Adaptation, fusion, compensation and contrast may well be configurational in character. Probably the considerations applying to the determination of thresholds, discussed in the next Section, are true of all sense modalities.

[25]This Section should be read in connection with the following three Sections, particularly 22. Here we find the basic considerations entering into all temporally extended configurations.

cross to long-section phenomena. Whatever the result of analysis, we must be able, if our psychology is to make any pretense at being systematic and complete, to deal with dynamic processes in time with no break in theory and without the introduction of assumptions *ad hoc* (memory images, memory color, unobservable acts). The solutions offered by the configurationists of the psychophysical problem rest upon fundamental divergences in theory which have important implications for the even more complicated problems of thought and action.

In this Section I shall therefore limit the discussion to several fundamental questions in psychophysics, leaving it for the later Sections to trace the consequences of the facts here established. To conceive the problems in clear-cut fashion we may formulate them as follows: (1) What is a comparison and how are relations perceived? (2) How can we explain the so-called false judgments, or rather, the lack of agreement between phenomenal and physical structures? (3) What does the differential threshold really mean?

(1) The real importance of the first question was appreciated by von Ehrenfels and Meinong, who devoted considerable attention to judgments of likeness and difference. But their solution had at least two grave defects, typical of later psychological theories: first, by attributing the comparison and the relation upon which it is based to the activity of a subject or an "ideal" process, they withdrew the whole problem from control and observation and the domain of the real,—in fact, for Meinong and his school, relations and judgments may be real or ideal, the distinctions finally merging into logic and epistemology; and secondly, they conceived the terms (or stimuli) which are related to be prior to the relations and somehow built upon or added to them, hence less essential, often capricious and optional. Thus Stumpf, in this vein, asserts that relations are perceived in the sensations, at least two of which must be given, but the relations are not as palpable as the relata,—wholly overlooking the fact that neither sensations nor isolated units of a connecting relation need be perceived as such in order to form the basis for a comparison; for a unitary structure may be perceived as a "step up" or a "step down" with no hint of the relation of "greater than" connecting the members of the configuration. Still other accessory hypotheses have been made in order to bolster the element-theories, which begin by assuming that each unit may be observed independently of the other: thus we are told that judgments may be influenced by secondary criteria in the form of secondary sensations which form transitional experiences hovering about the primary data (Jaensch); unnoticed sensations are responsible for judgments of "less" when the response should be "equal" or "greater"; or the memory image or images may have lost in intensity; finally, the "relational process" may be misguided so that psychophysical errors are due to errors in judgment (*cf.* Stumpf's distinction between relations of sense and judgments of precision).[25]

[25]For the original sources of the various points made in this and the following five paragraphs see: K. Koffka, Psychologie der Wahrnehmung, *Die Geisteswissenschaften*, Part 29, 1913, 712-716, and 796ff; W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psych. Forsch.*, 4, 1923, 115-176.

The configurationists approach the problem of relational judgments from a wholly different point of view. Relations and relata form a single, unified structure. The judgment is usually made in terms of the configurational properties of the stimulus situation, and not upon now one element and now another, the relation supervening somehow upon both. In order to make a simultaneous or successive comparison the things compared must be put into a unitary structure. Furthermore, a judgment involving a total structure and a judgment of relations of parts within a structure may present two wholly different phenomenal problems: to compare the sides of a rectangle and to form a judgment of their relations are obviously two different matters. To make a judgment requires more than a mere perception of parts. The parts must stand out in some kind of structure of their own, independent of the totality in which they are found. Judgments of relations depend upon an even more restricted phenomenal field in which various configurational transformations are necessary, depending upon the relations judged. Every problem thus creates a new set of configurational conditions which will modify the final judgment. The instruction plays a large part in determining the phenomenal field to be judged.

(2) We are now faced with the second problem, how to explain false judgments? How can it happen that the heavier of two lifted weights may be called the lighter, the more intense sound judged to be the weaker? Leaving aside for the moment constant errors due to temporal and spatial order, we may formulate the answer of the configurationists by reference to simultaneous and successive comparisons.

It must be admitted that the perception of a relation within a structure may or may not be given, depending upon such factors as set, disposition, habit and the like, which even the configurationists have not been able to dispense with. If the judgment reveals that the phenomenal and physical structures do not coincide, then we must explain the discrepancy. To say that two lines which I falsely report as equal were not equal and to explain the report by an error in judgment or by asserting that certain sensations were not noted is to assume the constancy hypothesis and involves one in the stimulus error. There is no phenomenological criterion of the length of the lines other than their appearance at the moment the judgment is made. It is therefore necessary to account for the judgment in terms of physical and physiological principles which apply directly to the observed phenomenal facts on the simplest set of assumptions. From this it appears that a given physical structure may give rise to a number of judgments based upon the phenomenal pattern. In any case the judgment must be amenable to (configurational) laws. The first assumption therefore concerns the *paired* character of the data upon which the simultaneous or successive judgment is based. If it can be established that the physical or physiological configurations possess properties either responsible for or paralleling phenomenal configurations, then we have found a *real basis* for all judgments and the solution of the problem is no longer to be sought in arbitrary suppositions as to what must be the nature of the processes responsible for the judgment.

We are now in a position to understand what false judgments denote for the configurationist. If, for example, one is required to judge which of two stimuli is the brighter or darker, the brightness of each stimulus is not abstracted from the paper or cardboard in which it inheres, nor is each stimulus considered necessarily by itself. Both stimuli, rather, form a single configuration, releasing configurational physiological processes and a phenomenal configuration. If there is sufficient objective difference between the stimuli to form a step up or down, the judgment will be "lighter than" or "darker than." If the objective difference is not enough to arouse a step-wise structure the judgment will be "equal," a homogeneous phenomenal field being the result. A fourth possibility remains: if now

one member of the structure predominates, now another, the judgment will be "uncertain." All judgments can therefore be deduced without the aid of additional hypotheses concerning the nature of conscious processes. Indeed, the speculations of Lindworsky, Jaensch, Bühler and others concerning the mental processes of Köhler's apes, far from giving us a simpler theory, actually attribute more intelligence to the animals than Köhler does. For as soon as we postulate memory images, "transitional sensations," "the noting of material relations," and the like, we must suppose that the animals consciously draw upon their past experience and thus make the required comparison.[27]

Turning now to successive comparisons we are confronted with essentially the same problems in far more difficult form. Effects of time and in time require the most painstaking research before a satisfactory conclusion can be reached regarding the temporal configurations. Time order has been found to be of importance *per se*: the differential threshold for lifted weights, auditory and pressure intensities, and taste qualities is lower when the stimuli are given in ascending order than when given in the reverse direction.[28] The duration of a stimulus may affect reaction time, the latter varying directly with the former. The length of a stimulus or the interval between stimuli is not a property (like its intensity) affecting it momentarily, but rather a factor which affects the totality and the judgment of the whole. Since the beginning and the end of a process are physiologically important points, characterized by marked changes in potential, it is important how far apart the stimuli are from each other in time. The nearer two stimuli are together, the stronger will their total effect be.

We have already dealt with the errors found in simultaneous comparisons and have now to investigate the errors made in successive comparisons. The usual explanation of the fact that more "greater" judgments are given in the ascending order, even when the second stimulus is equal to or less than the first, is that the first stimulus leaves a memory image which loses its intensity in time; and, since the comparison is made on the basis of the memory image, the weaker image of the first makes the second seem greater. But when it was found that observers gave "greater" judgments when no memory image of the first stimulus was present, this explanation had to be given up. Köhler, working with sound intensities, found that the second of two objectively equal stimuli is judged "greater" twice as often as equal; moreover, the greater the interval between the two stimuli, the larger is the number of "greater" judgments. There is therefore a true asymmetry in ascending and descending steps. The second sound is heard, not by itself alone, but as a rise or fall from the first. And since a step is more impressive than an undifferentiated field, the stepwise judgments will predominate wherever conditions are at all favorable for them. Here Wertheimer's law of *Prägnanz* may be at work: every configurational structure tends to become as sharply defined as possible. While a certain minimum of difference is necessary for the steps to be perceived, we find that an ascending order or a lengthening of the temporal interval between the stimuli is sufficient to create the proper conditions for a non-homogeneous or rising structure-function.

We may say with Köhler[29] that the tendency of configurations to be completed in a certain way, *e.g.*, in the direction of sharpness (*Prägnanz*),

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[27]Further discussion of this point will be found in Section 22 below. *Cf.* also K. Koffka, *The Growth of the Mind*, 1925, 205ff.

[28]W. Köhler, *op. cit.*; K. E. Leonnard, Ueber Reaktionszeiten bei verschiedener Dauer des Reizes, *Psych. Forsch.*, 4, 1923, 205ff.

[29]Köhler, *op. cit.*, But according to Köhler comparative judgments cannot be explained from the phenomenology of the comparison; for this

simplicity, or completeness, is a special case of the more general law that a given direction or end may impend in a phenomenal field; the phenomenal character of the field will include within itself certain goals or futurities (*Herkunftsorte*), although these may not be given in consciousness in concrete, definite contents. It is these configurational tendencies upon which the relational judgments are made, and not upon traditional sensations or other accessory criteria. It thus becomes evident why the investigations of thresholds and psychophysical work in general have been unfruitful: since measurements of sensory experiences were being sought, the sensory data having a more substantial existence than their relations to each other, the relations and whatever knowledge we might have gained of them were thrown outside the pale of consideration. To be sure, masses of statistics and quantitative results bear witness to the industry and perseverance of numerous investigators; but their interests did not lead them to a study of certain fundamental factors in the psychophysical problem which are of immense importance for the psychology of memory, recognition and the higher thought processes.

In throwing out time errors from memory, when it was found that no memory image was present at the time when the successive judgment is made, the older psychology neglected an important problem of memory. For if we examine the effects left by a stimulus, either in consciousness or (hypothetically) in the nervous system, we shall find a number of facts which call for explanation and whose explanation can help us with memory, recognition, and the dynamic processes in time. On the basis of his experimental work on successive comparisons, Köhler notes the following effects of a given stimulus: (*1*) it leaves a residuum (a dynamic process not to be confused with the static residua of associationistic writers); (*2*) after a number of repetitions with stimuli of the same kind (*e.g.*, sounds), an after-effect is left which changes the fate of subsequent incoming stimuli or their residua; (*3*) a stimulus of the same kind may be recognized after a long interval of time (recognition); (*4*) an absolute impression may be left by a given stimulus; (*5*) one may call up a memory image of a stimulus even after a year's time has passed. It may be thought, says Köhler, that five different after-effects are present in each case. Although recognition and reproduction do not follow the same laws, it is probable that the same kind of after-effect is at the bottom of these two memory performances. The absolute impression, which is also an accomplishment of memory, cannot be said to be identical with the after-effect at the basis of recognition and reproduction; but to assume that it is, is a more fruitful working hypothesis than to assume the contrary. And so Köhler, while confessing that he cannot reduce all five processes to a single psychophysical process, feels that we have the basis for a common set of assumptions to bring these various fields together. Thus the problem of relations in the psychophysical judgment may be of tremendous importance for the psychology of memory, thought and action.

(3) It will be remembered that we still have a third question to answer concerning the differential threshold. What is the explanation of the differential threshold and why does it exist? From the preceding discussion it becomes clear that the Weber-Fechner law applies to pairs of

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we must go to the nervous system. Here each stimulus leaves a modification (*Spur*) which has configurational properties, an inner tendency toward *Umbildung* in the sense of increasing definiteness and therefore possessing *relativ feste Bedingungen zukünftigen Geschehens* (146). Köhler presents three physiological hypotheses and seems inclined to accept the third as the best.

stimuli and their paired character and not to single sensations.[30] The old psychophysical view, therefore, that we are judging single sensations when making psychophysical judgments, is false; the judgment concerns the relata and their relation. In other words, while it is possible to collect quantitative data concerning the magnitude of experiences in various regions of the scale, the real meaning of the differential threshold is to be found in the light it throws on the problem of relations and *das Zueinander* of stimuli and their effect upon phenomenal fields.

Some of the configurational experimentation bearing upon the paired or structure-function effects of stimuli upon differential thresholds is worthy of note at this point.

Gelb and Granit investigated the color limen for a gray field when it appeared as part of a figure and when it formed part of the ground, the intensity remaining in each case the same.[31] The figure field was chosen both when it was darker and when it was lighter than the ground field. The fields were illuminated in part by lights of various colors, and the limens were determined for each. The results confirm previous findings concerning the fact that the color limen is higher for brighter fields than it is for darker. More important is the fact that the limen is different for a field when it is figure from what it is when the field is ground, even though its brightness value is unchanged. The limen for the figure field is greater than that for the ground field. The difference between limens for figure and ground is so much the greater, the greater the difference between figure and ground. In general it may therefore be said that the figure inhibits the perception of colors since its limen is greater.

Koffka had already pointed out that the color limen does not depend merely upon the brightness of the gray with which the color is mixed but also upon the total structure consisting of the tested field and its surrounding fields.[32] The greater the structure-difference between the brightness of the field tested and its background, the higher is the color limen and the more difficult is it to form a color structure. Gelb and Granit's results rest upon differences arising from figure-and-ground, not from differences in brightness, primarily because the brightness structure in their experiments remained the same.

These authors reject contrast effects and attention as explanations of their results. While it is true that a figure makes itself more impressive and that attentive factors are more favorable to the figure, still its limen is the higher. They believe that "distraction" worked equally against figure and ground, so that this cannot either be the causal factor in the situation. The explanation must be sought, they assert, in the fundamental distinction between figure and ground. The greater energy-density of the figure and its more concrete psychological characters go hand in hand with psychophysical processes that are opposed to any change. This is only a special case of the tendency of a figure to become and remain a simple, univocal, precise configuration, homogeneous throughout, resisting change. It must be remembered, however, that the results of these experiments cannot be generalized to include all figures, *e. g.*, figures lacking clearness.

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[30]The physical theory will be found in *Physische Gestalten*, 211ff; E. Becher, in his review of *Physische Gestalten*, presents a simplified account, see *Z. f. Psych.*, 87, 1921, 34. The perception of rhythm, time, and similar phenomena should come under this Section but lack of space forbids.

[31]A. Gelb and R. Granit, Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle, *Z. f. Psych.*, 93, 1923, 83-118.

[32]K. Koffka, Ueber Feldbegrenzung und Felderfüllung, *Psych. Forsch.*, 4, 1923, 176-203.

A more exact determination of the factors influencing color limens was made by Ackermann[33] and Eberhardt.[34] Ackermann determined the limens for different colors on inner fields with different surrounding gray fields, and for different inner fields with the same surrounding gray fields. A color was added to the inner gray field until the *O* could just notice and report on the quality of the color. The general conclusion to which Ackermann comes is that the color limens increase with the difference in brightness between the inner and outer gray fields. The critical-gray explanation of Hering and various other explanations do not suffice to explain all the facts. The theory which shall explain all must take account of many variables in the total situation. A further determination of these variables was attempted by Eberhardt.

Eberhardt wished to know what other factors besides brightness influence the color limen. She first investigated colored fields on neutral grounds. Here it was found that the color limen depends simultaneously upon three factors: brightness value of the inner field, brightness difference between the inner and outer fields, and brightness distance between the inner field and the outer field. All three of these factors must be controlled if we are to account for the color limens of gray fields. These dependencies hold for supraliminal colored fields as well as for the liminal colors.

Neutral fields on colored grounds were next observed. The question here is how the three factors enumerated above influence the quality and intensity of the contrast coloring in the inner fields. It is a well-known fact that contrast effects are best when brightness effects are least and the fields are most alike in brightness value. For the appearance of a contrast color, the intensity of the inner neutral field is of no account (when the ground is colored). It does influence the color tone of the contrast color, but in itself it is neither favorable nor unfavorable to the appearance of a contrast color. A brightness difference between fields diminishes contrast colors, which appear best when the brightness difference is reduced to a minimum, *i. e.*, when both fields are equal in brightness.

The limens of color and brightness for a field surrounded by fields of various brightnesses depend, therefore, upon the brightness value of the inner field, its brightness distance from the other two, and the white value of the inner field. The brightness interval between the fields influences not only the phenomenal effect of the brightness of the stimuli but also the appearance and saturation of the color aroused by contrast on an objectively neutral field.

We have been dealing above with factors within a configuration which exert an influence qualitatively upon the parts of the structure. We may turn to a study of conditions imposed from without a configurational structure, and note how these may modify internal qualitative characteristics of the configuration. In other words, we may say, with Koffka, that this story concerns the dependence of phenomenal field properties on external configurational conditions; or we may say that any given field is a part of a more complicated configuration when certain determinants outside its own boundaries modify its internal structure.

Koffka investigated fields which are phenomenally homogeneous when objectively they were not, and fields which appear heterogeneous when

[33] A. Ackermann, Farbschwelle und Feldstruktur, *Psych. Forsch.*, 5, 1924, 48-84.

[34] M. Eberhardt, Untersuchungen über Farbschwellen und Farbenkontrast, *Psych. Forsch.*, 5, 1924, 85-130.

objectively they were homogeneous.[25] An example of the first is found if one replace the broken black line of the Masson disc by a solid black line: a homogeneous white surface will be seen, whereas with the usual Masson disc one sees rings of varying brightness. Some *Os* do not perceive the homogeneous appearance of the pseudo-Masson disc owing to an "unnatural" attitude. It is possible to destroy the homogeneity by objective means also, namely by changing the conditions of the perception. Koffka measured the brightness of this field by equating it to the brightness of an objectively homogeneous field, and found that it approached the geometrical mean brightness of the latter.

In order to make an objectively homogeneous field appear heterogeneous Koffka placed a ring between a darker and lighter field. The middle field then showed a brightness gradient throughout its whole width. Such fields are used to demonstrate marginal contrast. Koffka does not accept the Hering theory as an explanation of this phenomenon, because it is stated in terms of the single effect of each field on the central field. So, too, with Mach's theory. These theories are also rejected for mathematical reasons, since a mathematical formulation of what must occur in order for the brightness gradient to appear shows that a darkening of one side of the middle field is not independent of a brightening effect in the other. The middle field either becomes homogeneous or does not, depending on whether the boundaries on both sides fulfill certain conditions. Stated mathematically, the two boundaries must change with respect to each other; and this change can be described by means of a parameter until now neglected, the arithmetical and geometrical means of the surrounding brightnesses. Koffka thus rejects the Mach-Hering explanations as atomistic theories, because they take cognizance of the effect of only one field at a time; the facts call for a more complicated function, which can be expressed only by taking into consideration the influence of both fields acting upon the middle field as a single unitary structure.

(21) *Configurational Aspects of Response Patterns*

Behavior or reaction patterns are dynamic configurations requiring time for their completion. The varied activities of the organism from the simplest reflex to the most complicated form of purposive behavior have a beginning, a transitional, and an end stage. From another point of view, every act appears as a figure upon the general background of the total organism. Even phasic, isolated acts, like the raising of an arm or the singing of a note, involve the tonic level of the whole muscular and nervous system. Every act modifies the existing state of the organism, leaving a more or less permanent effect upon it in the form of configurational dispositions or attitudes. The sum total of configurational dispositions and capacities within an organism determines the phenomenal and reaction patterns of which it is capable. Single part-responses neither run their course independently of each other nor are they linked by chance: the past, present and future needs of the organism combine to modify every single act in the economy of the whole. We may begin our consideration of the experimental work in the field of be-

[25] K. Koffka, *op. cit.*

havior proper by considering first the simplest motor configurations and the laws which govern them, and then proceeding to the more complex patterns.

Werner asked observers to draw simple figures, with eyes closed, under two different attitudes.[36] Under the non-membered attitude two lines forming a right angle may be regarded as a single notched line; under the other, as two lines enclosing a surface ("membered" attitude). What differences appear in the drawings, and therefore in the reaction patterns, under each attitude? Three possibilities are open: (1) the figure may be reproduced with little or no change, and this case we need not consider, since the given configuration is preserved. (Wulf called this the "conserving" tendency); (2) the lines may be bowed in so as to form a semi-circle (Wulf's tendency toward Nivellierung); (3) the lines may be bowed out, accentuating the angular appearance of the figure (Wulf's tendency toward *Prägnanz*). Under the non-membered attitude the figure drawn tends to become homogeneous, for here an assimilative tendency predominates: when the property of the whole dominates over the parts, the individual members lose their individuality and become absorbed in a more or less homogeneous structure. Under the membered attitude the angle is preserved and strengthened because the members are differentiated more and more from each other, in form, size and position. The membered attitude accentuates the dissimilative tendencies within a configurational structure. The configuration attains its fullest *Prägnanz* by the individualization of its members, *e.g.*, contrast effects in vision. Werner found that these general assimilative or levelling dissimilative or sharpening (*Prägnanz*) effects hold for auditory and speech phenomena.

The same laws of assimilation and dissimilation hold for the response patterns in speech and govern changes in accent, pronounciation and length of syllables. Werner's attack upon the problem of language-change stands in striking contrast to the associationistic studies made upon the same problem by other psychologists and philologists interested in the growth of language.[37] Werner considers words as structures, built about certain peaks or vantage points. Thus the word *getan* may be pronounced by accenting the *t* (simple centering) or by separating the syllables *ge-tan* (double centering). In the first case the peak is preserved and the rest of the word assimilates itself to it qualitatively. With the double centering there is a tendency to sharpen the members rather than to fuse them in a homogeneous structure. It is in these assimilative and dissimilative changes that we find the basis for changes in pronounciation, accent and tonal quality of words. These changes occur only when the parts are members of a single configuration and are not merely "neighbors,"—a view which is opposed to the widely prevalent assumption that changes in language are a result of association (frequency) with words of similar classes or categories. We thus see how the growth of language may be conditioned by configurational principles.

From the configurational point of view perception and action cannot be separated; the one flows over into the other. There is therefore no break in continuity between phenomenal and behavior patterns. In order to understand the reaction of the

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[36] H. Werner, Ueber das Problem der motorischen Gestaltung, *Z. f. Psych.*, 94, 1924, 265-272; F. Wulf, Ueber die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psych. Forsch.*, 1, 1922, 333-373. Wulf's work bears more particularly on memory and will be taken up in Section 22.

[37] H. Werner and E. Lagercrantz, Experimentell-psychologische Studien über die Struktur des Wortes, *Z. f. Psych.*, 95, 1924, 316ff.

organism to its environments, it is first necessary to understand the effect of the world upon the organism; hence an introspective report may throw light upon explicit behavior. Since the configurational concepts are not limited to static events in the nervous system, perception may be regarded as a stage preparatory to action. If an object appears "friendly", this *Aussehen* implies my reaction to it as well as my perception of it. Any cross-section of consciousness or behavior is meaningful only as it points toward a completed whole; so that we must introduce affective, appreciative and evaluative descriptive terms if we are to account for the complex behavior patterns in actual life. The meanings which point to action are more primary than the qualities in experience, since the latter involve isolation from a total situation before they are experienced. But such terms as ugly, beautiful, sad, gay, benevolent or malevolent imply dynamic properties of situations which lead from perception to action.

Meaningless reflexes as the units of behavior find little favor with the configurationists, smacking as they do of the associationistic-frequency theories according to which intelligent behavior consists of nothing more nor less than combinations of unit reflexes knit by trial and error. The summative theories must assume too many complications to explain even the simplest successful act: it is inconceivable, Koffka argues,[38] that a baby is furnished by nature with all the connections necessary for fixation within two or three days after birth. Since every fixation would require a different system of connections, if the object fixated were slightly displaced, the number of pre-determined mechanisms for successful fixation early in life would have to be enormous. Instead of the "pathway" hypothesis, Koffka regards the sensory, motor, and phenomenal areas as a single system wherein *what is to be seen* determines the eye movements as well as the afferent and efferent impulses. "The alteration of conditions (within the sensory and motor parts) must therefore take place in accordance with the greatest possible simplicity and equilibration of forces: and the principle of the greatest horopter will be found in harmony with this requirement."[39] The simplest reflex acts are therefore not mere mechanical products, the result of predetermined connections, but rather organic activities tending in certain definite directions.

The view that instincts are merely chained reflexes or blind compulsive activities is also given up by the configurationists. The facts upon which the mechanical theories of instinct founder are several: (1) there is persistence with varied effort until the goal or end is attained; (2) the movements continue in the direction of the goal to be attained and cease only when it is attained; (3) that each part-activity attains meaning only when viewed in the light of the whole or of the consummatory response. The problem which confronts the configurationists is to steer between the blind mechanism of the frequency and conditioned-reflex theories and the vitalism of Bergson, Driesch and others. The configurational solution consists in describing instinctive activities in terms of dynamic processes whose formal properties can also be found in the inorganic world. Thus the problem of instinct becomes a special case under the more general problem of the dynamic temporal configurations. We must regard all temporal configura-

[38] K. Koffka, *The Growth of the Mind*, 1925, 71ff.
[39] *Ibid.*, 80.

tions as possessing a beginning, a transitional and an end stage. Since the inner, emotional behavior fits the external behavior, it follows that "the emotions are dynamic phenomena conditioned in time" and can be treated with the instincts. "So long as the instinctive activity is incomplete, every new situation created by it is still to the animal a transitional situation; whereas, when the animal has attained his goal, he has arrived at a situation which to him is an end-situation.—Instinctive activity then becomes an objective mode of behavior analogous to such phenomena as rhythm, melody and figure."[40] If we seek for the nature of the nervous processes responsible for these activities and if we desire a parallel for these modes of behavior in the inorganic world, we can turn to Köhler's concept of "closure," which is defined as follows: "In every process which issues at all in an end-situation, independent of time (although it is possible to apply this to events occurring in time), the mode of distribution (of energy) shifts in the direction of a minimum of configurative energy."[41] The laws of simplicity, *Prägnanz*, and completeness, therefore, would apply to behavior patterns in time. Koffka's final word on the relation of configurationism to vitalism may clear up any misunderstanding which a superficial reading of the *Gestalttheorie* may inspire: "Although instinct is still a riddle, at least it is no longer one which forces upon us the acceptance of psycho-vitalistic principles."[42]

Voluntary activity does not separate very readily from non-voluntary activity, instinctive from intelligent behavior. It is probable that in voluntary activities, as distinct from non-voluntary, phenomenal fields undergo transformation, so that insight or understanding governs the part-activities in the light of the total pattern or situation. Thus Köhler's apes would fabricate a tool when the object and the goal were brought into the tool-goal configuration. But before this could occur, it was necessary for the object of perception to undergo a complete alteration, to be *seen* as the proper object to fill a certain gap or to satisfy a certain need.[43] It is not a question, then, of explaining each part-activity in and for itself and then of asking how the various responses become knit into a meaningful whole, as the associationistic and chance theorists do, but rather of explaining the solution and the sense it has for the animal. When the goal can be reached only in a roundabout way, as in Köhler's experiments, and each act has no meaning except as seen in the light of what the animal is after, we must infer that the end is the controlling factor in everything that the ape does.

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[40]*Ibid.*, 102f. [41]*Ibid.*, Koffka quoting Köhler's *Physische Gestalten*, 250.
[42]Koffka, *op. cit.*, 106.

[43]An example showing how necessary a perceptual transformation is before an adequate structure-function reaction can take place may be found in the following: if a table is needed for a certain purpose it may be entirely neglected by the ape if it stands in a corner, fitting closely into it. The table and the walls form such a firm structure that the ape is unable to separate the one from the other, and the table is neglected. The ape will tear a board off a box if there is a space between it and the other boards, otherwise the top forms a closed whole unsuitable for the solution of the problem. More will be said later concerning the transposition from one configuration to another.

The development of rational behavior consists, then, in the ability and choice of more and more adequate and intricate structure-function responses according to the general configurational principles enunciated above.

(22) *Recognition, Learning, Memory and Reproduction*

Current explanations of recognition, learning, memory, and reproduction rest upon the concept of the mental element and the mechanism of association. Repeated experiences are regarded as the causes of association, and the resulting association then becomes the moving force in recognition and reproduction. Sometimes the word 'habit' is used to denote both the mechanisms and effects of associative learning. Subsidiary aids in learning and memory performances reveal how association and the mental element go hand in hand: a given sound, taste or form is recognized or reproduced because memory images from past experience or other secondary processes accrue to a present perception and modify the percept or reaction accordingly. To be even more specific, let us take the problem of color constancy: colors maintain their pristine hue and saturation under illumination that should give wholly different effects; thus, a white sheet of paper looks white under illumination that should make it appear yellowish or reddish. The explanation commonly advanced for phenomena of this kind is that a "memory color" (Hering) of the object overlies the sensational presentation and preserves its erstwhile appearance. The image and the sensation fuse by "assimilation." Assimilation is enhanced by frequent repetition of experiences. Mental processes thus condition each other through frequent association. All memory performances reduce to the reproduction of elements which have frequently been attached willy-nilly to one another.

It is clear that "associations from past experience" can be made to explain anything in any way that one pleases; the treasure house of the past contains an infinite variety upon which one may draw. But memory images and past experience explain either too much or nothing at all; for they can be made, if manipulated with sufficient dexterity, to support any proposition or its contradictory. That we must penetrate behind the linkages formed in past experience is attested by experimental observation, seeing that two items associated a thousand times in the past may not be associated the next time one of them is presented. Why? Pure frequency theories afford no answer,—not so long as we remain faithful to them.

What have seemed to be the simplest assumptions (for did they not operate with the "simplest units" of experience or behavior and the simplest mechanisms?) really prove to be gratuitous in the face of the facts and hardly capable of experimental observation and verification. Let us revert once more to color constancy. Tests made with chicks only a few months

old show that when trained to pick the white (brighter) of two objects they still choose the (really?) whiter when it is in the shade and actually reflects less light than the darker nearby. It can hardly be said that these animals have a memory color of the white object as it appeared in normal sunlight. Even as able and careful a thinker as Bühler (I omit here the speculations of Jaensch, Lindworsky and others) is really forced to speculate more about the mental processes and memory capacity of the ape than Köhler does in his doctrine of "insight." Bühler hopes, by means of traditional associationistic-sensationalistic assumptions, to show that the ape possesses no structure-function reactions. Let us see what Bühler offers in place of Köhler's description: the ape is able "to feel itself towards the end-situation of attaining its goal, which is 'not difficult to explain theoretically as a memorial after-effect of successful pursuits of a goal in the past.'" Moreover, the animal is able to "note" material relations in the problems he is called upon to solve, just as he notes sensory contents![14] Both Koffka and Köhler have repeatedly emphasized the fact that to note relations and to perceive a configuration are two entirely separate affairs, involving different phenomenal fields and structure-functions,—probably in the case of the relation perceiving attitudes, very much more complicated perceptual patterns. Furthermore, it is a matter of pure speculation what the animal remembers from past situations, what he notes among the relations present, and how his mental processes work. The memory image and the associationistic assumptions formulated *ad hoc* have yielded few implications for further research, factual verification or prediction.

The problem of recognition reduces to a more simple scheme in the configurational psychology. Neither identical elements, nor old pathways nor former functional centers have to be recalled to life in order that objects, persons, or situations may be recognized. By hypothesis the configuration is a transposable form; only similar structures need be present, and not identical elements, when recognition takes place. Since experience follows configurational laws we should expect to find structures in all experience, and hence to find the formal conditions for recognition present quite universally. Both everyday observation and experimental results confirm this supposition. The tendency to identify nonsense syllables may be so strong as to make useless all attempts to induce a neutral attitude on the part of the observer or to get a purely mechanical response from him. To be sure, past experience plays a rôle in present perception and action,—but not by means of elementary residua associated to other units through frequent repetition. Configurational attitudes, dispositions and tendencies still play a large part in *Gestalttheorie*, but in radically different fashion from the older mechanisms. We are told that, once configurational structures have appeared in experience, their reappearance in the future is easier even though the objective conditions are less favorable. In accordance with the laws of completeness,

[14]Quoted from Koffka, *The Growth of the Mind*, 207. This whole section in Koffka's book, 205-230, should be read by everyone interested in animal psychology and the fundamental concepts of general psychology; some of the most acute criticism of traditional concepts will be found here.

Prägnanz, and perfection (to mention only the more important), a part of a configuration may facilitate the whole. Phenomenal and behavior patterns tend to "become as good as possible" and do not follow the laws of chance.

There are other factors, besides the form of a structure, which are important in recognition. Stated generally, they concern the properties belonging to the configuration as such. I can give here only one or two illustrations. Rubin found that a field is more apt to be recognized if it is seen twice as figure (or as ground) than if perceived now in one way and now in another.[46] Often, if the field had been seen as figure and later as ground (the stimuli being ambiguous figure-ground configurations), it was not recognized as the same. The difference between figure and ground is therefore not one of degree (frequency of association) but one of kind; figure and ground possess wholly different phenomenal properties. Numerous experiments show that when the formal properties of different sense modalities do not coincide, identical stimuli appear as different objects and may pass unrecognized.[46]

We may now turn to the phenomena of memory and learning. Here, too, chance-frequency theories have dominated experimental procedure and interpretation of results. If an animal cannot survey the whole situation in which his problem is enmeshed, the result will be a chance performance and a smooth learning curve. But trial and error, frequency, and associationistic theories do not explain the sudden drops or rises in the learning curve, nor for that matter do they alone explain any learning whatsoever; for additional hypotheses, such as the stamping-in effect of the pleasant consummation, must be brought to the support of the original theories.[47]

We come now to an important factor in learning, memory or retention. We may regard memory from the standpoint of what is or is not retained, and we must also consider the changes which occur in retained material after a lapse of time. The typical explanation here is also associationistic: frequent associates tend to strengthen one another, while the rest drop out. Köhler has shown that the apes retain structure-function reactions after a lapse of several weeks, while the absolute choices in which unit elements within a situation are learned last only a few days. The work of Lewin on association proves that mere repetition of associates does not play an important part in memory performances.[48] The laws of retention must be reformulated in terms of structure-function activities.

We have next to inqure concerning the changes in remembering or learned material. Müller's theory of memory changes may be contrasted

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[46]E. Rubin, *Visuell wahrgenommene Figuren*, 1, 1921, 28.

[46]*Cf.* von Hornbostel's findings, this JOURNAL, 36, 1925, 525f.; also Section 24 below.

[47]For the proof of this statement see Koffka, *op. cit.*, 153ff., 234ff.

[48]Lewin's work has important consequences for all the facts so far discussed in this Section; but I have reserved discussion of it until p. 50 below, when it can be brought in at length.

with the configurational interpretation.[49] According to Müller, the tendency or direction of ideas is a result of a law of memory by which remembered things lose their determinateness and become more alike as time progresses. He asserts further that images belonging to the same sense modality but to different objects converge toward a state of unclearness in which their differences are obliterated. This law, the "convergence principle," is supposed to possess universal validity. Wulf's investigation was designed to test both this theory and the associationistic structure upon which it rests.[50]

Wulf exposed simple figures for 5 sec. and more complicated figures for 10 sec. After intervals ranging from 20 min. to 24 hrs. the observers were asked to draw what they had seen. In comparing the reproductions with the originals, the fact was noted whether special properties of the figures were preserved, omitted, or sharpened. These configurational tendencies were designated as conserving, levelling (*Nivellierung* or *Niv.*), and sharpening or accentuating (*Prägnanz* or *Pr*). Extensive introspective reports accompanied the reproductions. The results show that with the exception of 8 cases there is always a clear deviation from the original in the direction of *Pr.* or *Niv.*, while comparisons of reproductions of the same original made at different times indicate a continuous change in the direction evident in the first reproduction. The quality of *Pr.* or *Niv.* could be recognized more or less strongly. At times the change was so marked that a new configuration was drawn, and the observer reported that he had no memory of the original. (The observers were good, bad and indifferent visualizers). Nor were the reproductions based on imagery: for (1) the copies were not copies of images; many of the observers had poor, fleeting or no imagery; and (2) often the image and drawing showed no correspondence when both were drawn.

The changes in content which appear after a lapse of time may therefore be said to be subject to configurational laws; *Pr.* and *Niv.* manifest uniform directional changes. Both may appear within the same reproductions, for they are not antithetical. And in view of the fact that these tendencies were not accompanied by affective states, they cannot be identified with Müller's affective transformations. Nor can the changes in the reproductions be ascribed to Müller's convergence principle, according to which ideas tend to become more alike as they lose in clearness during temporal intervals. The laws of *Pr.* and *Niv.* are special cases of the more general law of *Prägnanz*: configurational changes in memory, as well as in perception, tend toward the sharpest and best form possible, *i.e.*, every configuration tends to become as "good" as possible. In other words, two configurations either approach each other in symmetry and form (or the members of a configuration do this, in which case we have what Werner calls "assimilative" changes tending toward homogeneity), or their differences become accentuated so much that two new configurations are formed, (Werner's "dissimilative" tendency). We find thus that the accentuating, levelling and preservative tendencies may be regarded as exhibiting typical configurational changes in memory functions.

What an individual can reproduce represents his highest memory performance *par excellence*. Reproduction in one form or another is the test of the amount of memorization, retention, learning, and accomplishment. Since it was earlier assumed that these functions are nothing more or less than the result of

[49]G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, *Z. f. Psych.*, Erg.-Bd. 8, 1913, 509.

[50]Wulf, *op. cit.*, note 36.

association, it followed that the capacities behind them could be measured by excluding all meaningful factors and isolating the associative mechanisms. The upshot of all the work on memory based on these theories has been to collect statistics having little or nothing to do with actual learning, since in daily or professional life people do not memorize nonsense syllables. The most disheartening fact in the whole situation is that it now appears that, when all meaningful factors are ruled out and the other conditions required by the associationistic assumptions are fulfilled, no learning at all takes place, no associative bonds are established. The absolute failure of associational mechanisms (if there are any) demonstrates what it means to take the atomistic principles seriously and to evaluate them under exact experimental conditions. The work of Lewin shows that all of the so-called laws of learning depend upon more than frequency-linkages of separate items in mechanical fashion. Before beginning a discussion of Lewin's two papers, I quote his general conclusions based upon experimental evidence: the law of association as hitherto conceived is untenable, since the effects demanded by it are absent even though the conditions called for by it are present; and the law is stated in altogether too general terms (1) in giving the conditions necessary for the formation of an association, and (2) in leaving out of account the conditions necessary for the effectiveness of the so-called reproductive tendencies when they actually function in reproduction.[51]

We must first refer to the work of Ach, which was repeated by Lewin. The preference for doing a certain thing Ach called the determining tendency. Thus the stimulus *a* may leave an after-effect determining how we react to *b*. It was thought that the strength of this determining tendency can be measured by the so-called "associative equivalent," which is obtained as follows: *O* associates two nonsense syllables a given number of times; he is then instructed to rhyme or invert when one appears. *O* may then either carry out the given instruction or may reproduce the syllable formerly associated with the one which appears; thus the strength of the various tendencies may be measured. A reaction in which the association succeeds in the face of the rhyming instruction is called an "intended failure reaction." The strength of the association which is just sufficient to bring about the intended failure is the associative equivalent. In other words, the strength of the association can be measured by the number of repetitions necessary to establish a connection that will bring about an intended failure reaction.

Two kinds of activities may be called forth by a stimulus or instruction after a series of syllables has been "learned." After two syllables have been associated, the instructions may require that another syllable be reproduced instead of the one associated; this awakens a "heterogeneous" activity which works against the associative activity. Heterogeneous activities are supposed to cause a reproductive inhibition (*Hemmung*) and

[51]K. Lewin, Das Problem der Willensmessung und das Grundgesetz der Assoziation, *Psych. Forsch.*, 1, 1922, 191-302; 2, 1922, 65-141. Ach's work will be found in his *Ueber der Willensakt und das Temperament*, 1910.

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to lengthen the reaction time. A so-called "homogeneous" activity is aroused when the instructions and the associative tendency established by the learning of the two syllables work in the same direction. Homogeneous activities are supposed to produce shorter reaction times.

The *O*s were shown syllables and told to learn them. Two series of 16, two of 8, three of 4, and six of 2 were given. From these two series two similar sets were chosen. Every set was reported six days out of seven, so by the end of the fifteenth day every series had been repeated 270 times. Meaningful aids were not used in learning, although rhythm and complex building were not avoided. The *O*s preferred the series of 16 and 8 syllables because they could be put more easily into rhyme. A series of neutral syllables was used for comparison purposes. This series was memorized in haphazard order, the learning being done for the purpose of avoiding the effects of "strangeness" in a neutral set.

The tables show that, in spite of 270 repetitions and against the expectation of all, there was no heterogeneous activity, no determining tendency established for an association to persist; the reaction time for syllables learned together was approximately equal to that of the neutral syllables which had no associates. The *O*s repeated the associated syllables, to show that they knew them as well as before. The exceptions to this general statement were due to the fact that the *O*s tend to identify syllables, to see if they can be recognized, hence to put them into a structure. The identification process cannot be said to be the result of association, because it might occur with syllables which had not been associated. Under express instructions to the *O*s to be neutral, there was no difference in reaction-time between syllables which had been reproduced together and new syllables. The law of association, according to which an appearing associated syllable tends to call up its non-presented pair, as well as the concept of associative equivalents, falls to the ground.

The lengthening of time, reported by Ach and ascribed by him to the effect of determining tendency, was absent. Even when asked to repeat "the first thing that came into their heads" after seeing one of the associated syllables, the *O*s did not give the syllable which had been associated with it several hundred times. Where then is the binding effect of association pure and simple? Syllables rhymed with syllables beginning with gutterals were rhymed just as quickly with labials as were syllables practised with labials. With passive instructions, in no case did a single response give a rhyme learned 260 times with the stimulus. No tendency toward the reproduction of the second on the sight of the first was present!

It is beyond the scope of this article to enter into the refined experiments, the checks, and answers to objections cited in Lewin's paper; for all this the reader must go to the original, which is more convincing than any abstract can be. We have now to determine what, if any, are the real conditions under which so-called associations are established and become functional. To determine this Lewin tried to produce associations that would be effective in manifesting themselves as reproductive tendencies, retarding the heterogeneous activities, and accelerating the homogeneous activities. Eight series of syllables were used: 4 series were constantly rhymed, 4 were inverted constantly. They were exposed visually and read aloud, after which the *O* gave the response. The results in favor of "association" are now as startling as those previously against it: although the constantly inverted syllables had been repeated only 8 times, the first time they were called upon to rhyme them there was a failure reaction, and the second time they were asked there was considerable increase of the reaction time. And with only 11 repetitions of the constantly inverted syllables, the exposure of a constantly rhymed syllable led to an intended failure reaction and was not noticed as such. It was possible to get an intended failure reaction with only 4 associative repetitions. Hence the effects

demanded by the laws of association were obtained: intended failure reactions, inhibition or facilitation of the heterogeneous activity and acceleration of the homogeneous activity, all appeared after a small number of repetitions.

The causes for the difference between the results of the first and the second paper were revealed by the introspective reports of the *Os*, which show that mere frequency does not suffice to explain the so-called "associative" effects. The instruction, attitude and motor response form a single configuration which must be analyzed separately. The instructions for a given reaction may be framed in divers ways and carried out in divers ways. It makes a difference whether an *O* is asked to "rhyme or invert" a given syllable or to "name the usual or earlier (rhymed or inverted) syllable which follows the given stimulus." The former instruction sets into operation a configurational pattern in which the end or goal releases and controls definite activities, while the second really leaves it to chance whether or not the associated syllable will be reproduced. *In this way the response can be controlled, and not by the frequency concept of the associationists.* The difference does not therefore consist, as the associationist would say, in the strength of associations, but in the fact that in the one case the *Aufgabe* is to *reproduce* in a certain way, in the other it is *to build a new syllable.* The number of associations after a few repetitions is of little account; rather, certain readinesses-to-respond in a certain direction determine what will be reproduced. These tendencies toward response need not be conscious; they may be related to the stimuli in very definite ways; and they form a relatively firm structure with the actual performance.

A comparison of neutral activities with definite ones also shows that association *per se* is not sufficient to bind together two contents. Admitting that it is impossible to put oneself in a wholly neutral attitude (which works also against the associationists!), it is possible to give neutral instructions and then to determine by introspection how far the *O* has been able to comply. In this way something approaching a neutral attitude may be appromixated; and the results show that the mere repetition of syllables sets up no reproductive tendencies whatsoever.

It appears, therefore, that out of the dross of the associationistic experiments some very important knowledge can be gained concerning recognition, memory, learning and reproduction, provided that we approach the facts with sufficient critical and constructive implements. Whenever the atomistic theories are taken seriously at their face value and without bolstering hypotheses, it is found that they do not suffice to explain the most elementary facts of perception, memory, or behavior. The supposed linkage of meaningless elements through mere

repetition simply does not occur; there must be some structures already present in the organism which are ready to respond mechanically if nothing else is required of them. It is necessary to introduce long-section, functional concepts for the facts of insight, true learning and successful accomplishment; and this must be done with a minimum number of assumptions concerning the memory processes, the intellectual abilities, and the mental equipment of the human or animal subject. Only thus can a satisfactory approach be made to the problems of capacity and function, whether in perception, recognition, memory, or the higher thought processes.

(23) *Configurationism and the Higher Thought Processes*

One cannot approach the higher thought processes without bringing to mind the work of Watt, Marbe, Bühler, Messer and other members of the now defunct imageless-thought school. What at first promised to be a new mode of attack on the problem of thinking finally proved to be little more than the old association psychology supplemented by *Aufgabe*, determining tendencies, relations, and a new mental element joined in the usual fashion to the old. The potential *Wissen* of Bühler, Messer's predicative relation and transcendent meanings were all entities incapable of psychological demonstration or control. The use of unobservable entities and mechanisms in no way helped to support a theory which by its very nature must fail to explain orderly, rational thinking since it reduces all to the chance connection of meaningless units.

As Titchener and Bühler have pointed out,[52] the problem of the higher thought processes contains a number of subsidiary problems, many of them having little to do with one another. Judging, inferring, comparing, abstracting, willing, understanding, believing, valuing and appreciating have all been included under the rubric of thinking. It will therefore be necessary to restrict this discussion to what may be called logical thinking, thinking in which a certain goal or end is deliberately sought. We may put the problem thus: "How do we reach our goal in logical or creative thinking?" The typical answer of the associationists may be gleaned from Müller's constellation theory. According to this theory the goal is merely a constant in the chaos of reproductive elements and tendencies. The product of thought finally emerges from the reproductive tendencies because this particular item has been the most frequently associated in the sought-for connection. But on the basis of this theory

[52] E. B. Titchener, *Experimental Psychology of the Thought-Processes*, 1909; K. Bühler, Tatsachen und Probleme zu einer Psychologie der Denkvorgänge, *Arch. f. d. ges. Psych.*, 9, 1907, 297.

it is impossible to explain how a new problem is solved at the first attempt, how a new product may be the result of thought, or why the significant term or relation which holds the key to the solution is seized upon.

The logical analysis of the associationist is responsible for the plight he gets into; for, having begun with elements having no relation to one another, he must by some means connect them. The configurationists answer the question of thinking by asking how thought progresses from the beginning to the end of a problem. The historic antitheses of sense and intellect, sensation and thought, do not exist in configurational psychology; hence there is no need to build one on the other or to derive one from the other. Memory and thought saturate perception, and perception implies memory and thought. For thought to progress along logical lines, it must proceed within a structure, and this structure is very often furnished in perception. The relations of objects to one another are given in phenomenal configurations won in perception. A change in the object of perception may mean a change in the whole course of thinking, since rational insight demands the perception of the structure of the whole. Fruitful thinking consists, therefore, in approximating, either in perception or thought, to the *sachlicher Zusammenhang* of configurational structures. Configurational connections are not the results of frequent associations or blind juxtapositions, but are inner bonds imposed by the demands of the total structure. Once this structure is seen, the parts must follow one another in a certain way.

The concept of closure can be applied to thinking. Questions demanding thought arouse configurational processes which are incomplete and call for closure.[53] The answer thus "sticks" in the question. A good question and a good answer are not matters of chance: they fit each other as the key and the lock. Once the form suggested by the question is apprehended, there is a sudden *Einschnappen*; the inner bond appears, baring the structure, and the configuration is completed. It is this sort of process which is responsible for the sudden drop in the curves of animal learning,—a fact which trial and error, and frequency theories have never been able to explain.

It must not be thought, however, that all thought configurations are direct functions of perception. Experience leaves configurational dispositions which may be set into operation by part-situations. Again, the inner configurational conditions may change the organism so as to modify perceptual configurations and one's response to them. Thus puzzle pictures attain new perceptual properties and meanings when new inner configurational dispositions are aroused. To speak in such cases of mere associative-linkage is to neglect the specific properties by virtue of which the total

[53]For a simple explanation in English of Köhler's concept of closure, see Koffka, *The Growth of the Mind*, 103.

situation is transformed both in thought and perception. The extent to which configurational transformations may affect our phenomenal and rational worlds is almost unlimited. The universal causality of the savage, compared with the restricted and refined concept of causality held by the modern man of science, amounts to a re-ordering of the whole perceptual, emotional and rational worlds of the latter in a way unknown to the former. The relations of facts within firm thought structures are based, not upon the savage's frequent associations and his (Humian) view of causality, but rather upon laws which modify and order the successive data of perception and thinking in the light of the structures to which they belong.

The question still remains concerning creative thinking, or if there is an original product in thought. This problem (that of the petitio in the mode *Barbara*) has become a classic in logic, which asks: Does the conclusion of the syllogism involve anything new and not contained in the premises? Logicians generally answer both No and Yes. Logically there is nothing new, psychologically there is a new product. The logical problem has thus been transferred to psychology, which must explain how thought can tease something new out of existing propositions. Wertheimer has endeavored to give an answer in terms of configurational theory.[54] His solution may be summarized as follows: Creative thinking consists in the transposition of a member of one configuration to another. Thus members of two different configurations (the premises) are identified as belonging to a common (new) configuration (the conclusion). Membership within a new structure possessing its own peculiar properties sets the transposed members in a different light. Insight depends upon the ability to break up existing configurational structures and to make fruitful transpositions from one configuration to another. A concrete illustration may make this statement clearer. When in need of a tool for reaching food, the ape will use whatever objects are within his ken, putting food and stick or branch within the same configuration, the stick becoming a new phenomenal object once its tool-properties have been recognized. But if the stick or branch is part of a tree, *i.e.*, has not been separated from the tree of which it is a part, although the ape is familiar both with sticks as tools and branches as parts of trees, he will not tear the branch from the tree and use it as a stick-for-getting-food. Certainly the animal sees the branch, but the phenomenal field of which it is a part presents too firm a structure for the ape to break up. The existing configuration must be changed before transposition can take place and the branch be put into the tool-food structure. In other words, to see the branch on the tree as a stick-for-food is quite beyond the ape's visual and thought powers.[55]

Thinking necessitates not only the perception of a given structure but also the destruction, often, of old custom-bound configurations in order that new structures may become dominant. Many of the stupidities and "foolish" mistakes of students in the classroom and of adults without may be due to the inability to transpose from one situation to another or to see things as fitting and proper members of new configurations. The ability to transfer members of one configuration to another may be a definite structure-function which in pathological cases

[54] M. Wertheimer, *Ueber Schlussprozesse im produktiven Denken*, Berlin, 1920.

[55] The resistance of configurations to change is a fundamental fact of "good" structures. Factors working for or against the preservation of structures must be further investigated, since it is not wholly clear even in the simple phenomena of movement.

often degenerates and which probably never develops among those of inferior intelligence.[56]

Finally, the fact must be mentioned that thought structures are real in every sense of the word. Thinking logically is not the result of blind chance, of frequent associative connections, or a matter of caprice. For the results of thinking depend not only upon thought, says Wertheimer, but also upon the objects thought about. If all connections were matters of chance or caprice, objects could never mean anything definite. The inner necessity pervading the entire structure does not come from the mere relation of part to part, since the whole is more than a sum of parts in relation, but rather springs from the peculiar nature of the whole which is more or less autonomous. The course of ideas is therefore determined by configurational laws effective throughout the entire range of human thought.

(24) *Contributions to the Psychology of the Abnormal*

It has been found in studies made with pathological subjects that many grave defects in the ability to perceive, think, remember and act may be due to lesions which affect the configurational processes necessary for the normal functioning of the organism. The configurationists insist upon two fundamental propositions with regard to the principles upon which a psychology of the abnormal must proceed: (1) configurational laws govern the deviations from normal conditions, while, conversely, pathological states must be regarded as the results of a breakdown in the mechanisms responsible for configurational phenomena; (2) in cases of partial degeneration, normal and abnormal do not function summatively side by side, but rather new structures are formed which obey configurational laws and function "para-normally." Thus the normal and abnormal facts of human behavior must be deducible from a single set of assumptions linking both fields. Much light has been thrown upon the operation of normal configurational processes as a result of the investigations by Gelb and Goldstein, Fuchs, Benary and others in the field of the abnormal. But since the results are by no means capable of univocal interpretation, I leave a critical discussion of the issues raised concerning the very foundations of *Gestalttheorie* until the next paper, and shall discuss here only what seem to be the main contributions of the configurationists to the psychology of the abnormal.

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[56] *Cf.* the argument of the next Section, where the results of experimentation with pathological subjects are quoted. An application to the problems of mental testing and intelligence will be found in *Naive Physik* by O. Lipmann and H. Bogen, Leipzig, 1923.

Experiments with pathological subjects reveal gross defects in the ability to perceive, and to react to configurational situations.[57] Evidently the mechanisms responsible for the reactions to finer and more intricate structures cease to function, and the patient seems to have retained only a capacity for the simplest and most elementary (primeval) perceptual and motor configurations. One of the main factors in recognition appears to be form. If the form or structure of an object is not apprehended in perception, the object will not be recognized. If the patient cannot transpose the form either from past to present by means of memory, or from one sense modality to another, some of the apparently most simple functions may be lost: the patient cannot name or pick out common objects, fails to understand the significance of relational situations, and seem generally deficient in intelligence. One patient could not recognize forms by means of vision, owing to lesions in the visual areas of the brain, with the result that if he was to read or name a simple figure he was obliged to build the forms by means of head movements which follow the outline of the parts. But he could not make the simple transposition of form from one sense modality to the other: an object recognized by means of head movements (eyes cooperating) was not recognized when laid on the skin.[58] It appears, therefore, that the criterion of transposability, while not necessary, is an important criterion of configurational responses.

It has also been shown that pathological defects may involve only specific configurational responses and not the general intelligence of the patient. Indeed, some configurational mechanisms may remain intact, while others owing to the injury may function in an altogether new manner,—all of which would remain inexplicable on the basis of associational and atomistic theories. In this connection Benary has found even such mechanized responses as the operations involved in counting require configurational dispositions which may be disturbed. His subject could not begin to count in a series unless he began with one; he could tell that 1000 is greater than three, because the step between these two numbers is great, but when asked if seven is greater than three he had to pause before giving an answer. Every step in the ordinary arithmetical operations had to be painfully carried out and noted before he could proceed. He could not see that two times six and three times four, in giving twelve, had anything to do with each other. His ability to perceive identity and difference in different examples of the same kind was gone. He could not immediately perceive a total structure; usually it was a matter of inference from the parts.[59]

The patient cited above and other patients showed repeatedly the importance of structure for the processes of thinking, recognition, memory and action. If, owing to some pathological defect, the patient was unable to make use of a set of *Anhaltspunkte* by which he could orient himself, he set about searching for such support before attempting the real solution.[60] Without definite configurational structures the patient is lost, un-

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[57]The chief contributions to the psychology of the abnormal have come from Gelb and Goldstein and their co-workers. Their papers appear under the general title of the book quoted in note 21, *Psychologische Analysen hirnpathologischer Fälle*. In this series will be found papers not only by Gelb and Goldstein but also by Fuchs, Benary and others.

[58]This patient has been made the subject of intensive study and has been described in the book cited above and in a paper by W. Benary, Studien zur Untersuchung der Intelligenz bei einem Fall von Seelenblindheit, *Psych. Forsch.*, 2, 1922, 209-229.

[59]*Ibid.*

[60]See Ueber den Einfluss des vollständigen Verlustes des optischen Vorstellungsvermögens auf das taktile Erkennen, in *Psychologische Analysen.*

able to perceive, think or respond clearly and adequately. Transformations of complicated configurations into simpler configurations of which the patient is capable are often found. Thus one patient, upon being asked to put all objects together which belonged together, made a collection based upon apparently no ordering principle. When he explained that he put all wooden objects in one pile and all iron objects in another, his response became intelligible to the experimenters.

Where some normal functions are retained, the normal and abnormal do not function summatively or independently of each other. The normal and abnormal portions of the nervous system and receptor-effector organs show a mutual interdependence which may amount to a new structure-function.[61] This new structure-function will obey configurational laws of a more simple or a more complex nature, depending upon the particular facts involved. Fuchs, Gelb and Goldstein and others have found that objects exposed so as to fall within normal and hemianopic areas may be completed, seen as wholes (*totalisierende Gestaltauffassung*). The objects may be displaced toward the normal portion of the retina, or rivalry may appear between the normal and pathological areas with accompanying changes in the phenomenal data. Such facts as these, the configurationists assert, cannot be gleaned or explained by either a point-to-point investigation or a summative theory of vision.

One more important point remains to be mentioned regarding the configurational theories. If perceptual fields are important for normal behavior and cannot be omitted from a complete description of organic activities, a knowledge of the phenomenal world of the abnormal subject is even more important for the understanding of his behavior and of the defects underlying it. Gelb and Goldstein found that pathological subjects could hide gross defects in behavior which were revealed only after eliciting careful phenomenological reports on what the subject experienced. These investigators point out that there has been too much inference regarding the phenomenal world of the abnormal, with the result that the descriptions and the theories have suffered. In order to know how and why one often reacts as one does to a particular stimulus it is necessary, as Koffka points out, to know how the stimulus affects the individual. Certainly the behavior of the insane and of many borderline cases would be inexplicable did we not penetrate beneath the external phases of their behavior. "That a patient can react to a given object and is even able to give its right name does not yet prove that his perceptual processes are normal. Explicit behavior may cover gross defects which will appear only under the refined conditions of the tachistoscopic experiment."[62] Thus Gelb and Goldstein, whom I have just quoted, discovered that one of their patients could read only so long as he could make head movements. Phenomenal visual forms were entirely lacking. Only experiments designed to test the patient's phenomenological data and reports revealed the nature of his defects.[63]

(25) *General Implications of Gestalttheorie*

The fundamental concepts of any science have important implications for life and for philosophy. It has been impossible

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[61] The evidence will be found in Fuchs' articles in *Psychologische Analysen* and in *Psych. Forsch.*, 4, 1923, 38ff.; 6, 1925, 187ff., from which the following facts are taken.

[62] Gelb and Goldstein, *op. cit.*, 7.

[63] D. Usnadze has proposed a configurational theory of paranoia which I omit since it does not readily fall within the outline of this Section. See Versuch einer Theorie der paranoischen Eigenbeziehung und Wahnbildung, *Psych. Forsch.*, 5, 1924, 1-23. This article also contains some interesting hypotheses for social psychology.

in these articles to enter even into the several fields of psychology and to point out what configurationism has there to offer, to say nothing of any thorough treatment of the fields outside psychology proper,—which would be beyond the competence of the present writer. The translation into English of Koffka's and Köhler's books has made a detailed discussion of comparative, genetic and educational psychology unnecessary. The implications of the configurational psychology for the various applied psychologies have already received the attention of those interested in mental testing, in intelligence, in group behavior, and the like.[64] I propose in this Section to review very briefly what seem to be the main points of contact between configurationism and the physical sciences and philosophy.

It is a fact worthy of comment that the most thorough single theoretical presentation of configurationism should bear the title of *Physische Gestalten*, should make an appeal to biologists and physicists as well as to psychologists, and should undertake to furnish a single set of assumptions for three different sciences. Such a work does not fall far short of being philosophical in its import (I wonder if philosophers are aware of its existence?), and cannot help but heal many breaches both within and without psychology. Certainly if physiologists and biologists accept the argument of the book, the behaviorist will be unable either to pride himself on possessing the only set of scientific assumptions for psychology or the only point of view in harmony with the "objective" sciences.

The new physical and physiological theories bring psychology and the "objective" sciences closer together at points where the former must needs lean upon the latter. The deduction of Weber's law by Köhler on the basis of physico-chemical principles, the derivation of the properties of the figure from the conditions obtaining in the physical fields, show how psychology may emerge from the slough of the private datum in a manner acceptable to all. Yet the psychologist can deal with phenomenal data without having resort to the circumlocutions and arbitrary decisions of the behaviorist. The insistence upon actual observation and experimentation by the configurationists must result in a righteous impatience with all the logicizing, philosophizing and hypostatizing now rife in our science. And if, as Planck is reported to have said, the real in science is the measureable, the configurationists have demonstrated anew that psychology is a real science, dealing with real phenomena, and possessing data subject to real laws.

But it is not psychology alone which may profit by the new point of view—physiology, biology and philosophy have ghosts of their own to lay. The two sciences are still haunted by

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[64] *Cf.* F. Wagner, Das Problem der psychischen Strukturen, *Z. f. pād. Psych.*, 24, 1923, 193-208; Lipmann and Bogen, *Naive Physik*; R. M. Ogden, The Need of Some New Conceptions in Educational Theory' *School and Society*, 18, 1923, 343ff.

spectres of atoms in meaningless conglomeration. Biology and biologists are divided on the problems of mechanism and vitalism, the inheritance of acquired characters, and other questions which might well be attacked from a fresh point of view. Koffka has given us new light on the question of nativism and empiricism, so that the problem, *e.g.*, of inherited or acquired eye-movements bears a more inviting aspect.[65] Similiar problems in the other sciences might be revivified and might acquire new significance if a more fruitful attitude could be taken toward them. Köhler has pointed out some of the implications of his theories to physiologists and biologists.[66] They are, briefly, as follows: Local processes may be influenced by the needs of the total organism; parts function with respect to, and in the economy of, the whole. The almost providential adjustment of organisms to environment does not require either vitalistic entelechy or blind mechanism, provided one admits the reality of dynamic processes extended in time and governed by definite laws. The conditions which affect the total organism must receive their due consideration just as the minute factors within it do. Certain concepts current in physics find fruitful application in the biological sciences (including psychology). Organisms tend toward equilibrium, usually represent minimum energy distribution systems, and possess several degrees of freedom,—these and other physical descriptions enable us to envisage total organic processes in scientific terms free from metaphysical bias.

When it comes to the implications for philosophy one must tread warily; for fundamental assumptions are always, in last analysis, based upon certain philosophical presuppositions. But I take heart in approaching this field because even philosophy itself must bow to experimental findings, and philosophers themselves are often not averse from accepting a pragmatic position with regard to certain problems. Strangely enough, the philosophical aspects of many of the configurational doctrines were noted before they were brought into the laboratory,—Meinong's psychology merges into logic, epistemology, and theory of value; Husserl and Dilthey, and more recently Driesch, have discussed the importance of total, organized wholes. At least certain aspects of *Gestalttheorie* (in their logical form) are not unknown to students of philosophy. Gelb went back to Plato in his discussion *Ueber Gestaltqualitäten.*

It is perhaps not too much to say that a good deal of the associationistic-atomistic psychology has been handed down from the *arm-chair* psychology of the English empiricists and those whom they influenced. The easy, common-sense analysis

[65] K. Koffka, *The Growth of the Mind.*, 1924.

[66] W. Köhler, Gestaltprobleme und Anfänge einer Gestalttheorie, *Jahresb. ü. d. ges. Physiologie*, 1922.

found in Locke, Berkeley and Hume has influenced the laboratory and the textbook of psychology far, far too much. The fault is not all to be laid at the door of the empiricists—Kant divorced matter and form and introduced the categories to get them together again! But the work of Sherrington on the integrative action of the nervous system, changed conceptions in physics and chemistry, and the latest developments in mathematics and modern experimental psychology all attest the fact that the philosophy of the past has not been altogether right in its scientific foundations. On the other hand, many of the sciences, and psychology among them, have much to offer philosophy if her world view is to be adequate to the newest discoveries. The configurational psychology has swept away the psychological foundations of several current theories of space and time and cognition, and some logical and ethical superstructures.

I can point out only one or two concrete examples of what is meant above. Reversing the Kantian position, the configurationists assert that forms or complexes are real, constitutive, exerting effects which can be predicted, controlled and measured. They can no longer be regarded as the result of special mental acts (or categories). The fact that phenomenal and physical configurations do not always coincide is not cause for attributing all forms to a mental creation or making the *reductio ad absurdum* of the idealistic logic,—solipsism. The number of possibilities for correspondence or lack of correspondence between phenomenal and physical structures is finite (just four), and we are beginning to know something of the laws governing them: a physical aggregate may be truly structured and arouse a phenomenal structure; physical structures may be paralleled by phenomenal fields lacking structuration; an unordered physical aggregate may be paralleled by either phenomenal structures or phenomenal fields lacking structuration. Correspondence depends upon a number of conditions both within and without the organism. In any case, whatever is found requires no special mental categories or acts whose locus is at present unknown to science.

Hume's problem does not exist for the configurationists. Indeed, one is tempted to believe that it was created by the assumptions with whichhe started, and that hence his logic has never been impugned. But the configurationists do not begin with the Humian assumptions or the psychology upon which many of them are founded. There are no absolutely unordered data in perception which must needs be associated in order to bring lawful, meaningful connections out of chaos. Where did Hume find these isolated bits of experience which for him constituted the world? The Humian psychology and philosophy lead to the nihilistic position that there are no real phenomenal or physical systems or structures. Observation disproves Hume's assertions: associative and productive processes as he conceived them do not form the basis of the world order. It is, of course, admitted that in no case do structures arise without necessary and sufficient conditions. On the other hand, the opposed view that all events are so intimately related that no division can be made within the universe is equally untenable; for science is interested in part-systems which can be isolated for purposes of measurement and control. An universal causality is as unfruitful as the Humian denial of any causality.[67]

[67]An excellent discussion of this problem of isolation in science will be found in *Physische Gestalten*, ch. IV. on the problem of independent systems.

There still remain the laws of science. Wertheimer would say that they more or less embody real connections which are not arbitrary and which are founded upon *sachliche Zusammenhänge*. The quantitative laws and formulations of science cannot be juggled in any way one pleases and still remain true to the facts. Once the equation of a curve is given, every point in it is lawfully determined, and interpolation and extrapolation must follow the law of the whole if it is to be preserved. We may change our units of measurement, the mathematics we apply to our data, but this is not a matter of caprice on our part; for, after all, what we are trying to do is to find out "how the dice are loaded." True, no theory absolutely encompasses the facts, and hence the best we can do is to get approximations, —but there are bad, good and better approximations, and the progress of science consists in formulating better approximations on the supposition that there is something to approximate.

So configurational systems, wherever found, have their laws. No stark mechanism, pre-established harmony, vitalistic principles or mental impositions are necessary to explain these structures, whether found in the phenomenal or physical domains. The whole possesses an autonomy of its own which is nevertheless subject to definite laws. Mind is thus not set over against the external world, but rather finds its place in the world. It is possible, therefore, to have a monistic science without throwing the baby out with the bathwater as the behaviorists have done, or accepting the extreme idealistic position which makes of mind an unique phenomenon.

THE VISUAL APPREHENSION OF MOVEMENT UNDER SUCCESSIVE RETINAL EXCITATIONS[1]

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I. INTRODUCTION

A. *Historical setting.* Within the past decade psychology has witnessed the development of the doctrine embodied in the *Gestalt-Theorie*. The immediate origin of the doctrine and likewise the occasion for many of the experimental studies dealing with it was Wertheimer's *Ueber das Sehen von Bewegung* (**49**). Since the appearance of this study most of the publications upon the *Gestalt* have attempted either amplification or substantiation of Wertheimer. The contribution of Wertheimer himself to *Gestalt* and to the literature of visual movement lies not so much in his descriptive expositions as in his physiological assumptions regarding the cerebral explanation of the experience of movement.

The perception of movement is the most investigated and the most important, at least from a methodological standpoint, of all the perceptual *Gestalten*. It has furnished most of the experimental support of the theory in the writings of Wertheimer (**49**), Koffka (**20**, **21**, **22**, **24**), Kenkel (**16**), Korte (**26**), Lindemann (**28**), Cermak (**6**), Stern (**22**), and others. We shall duly consider these and other studies in so far as they are relevant to our own investigation.

To Stampfer (**43**) must be attributed the construction (1832) of the first stroboscope and the first artificial production of movement independent of the actual displacement of a physical object. His apparatus, which

[1]The author makes his acknowledgments to Professor Bentley under whose guidance the research was carried out.

he called a "stroboscopic disc", was quite simple. Upon one side of this disc an object was drawn in various successive positions. When viewed in a mirror through radial slits in the periphery of the whirling disc this object was observed as in continuous natural motion. Almost simultaneous with, but totally independent of, Stampfer, Plateau (37) constructed a similar apparatus which he variously called the "Phänakistoskop," "Phantasmaskop" and "Phantaskop." By means of it Plateau was able to produce illusory movements of the same character as those produced by Stampfer's disc. An instrument more significant for experimental purposes was constructed by Horner (1834) and named the "Daedaleum" (15). A hollow rotating cylinder with vertical slits was mounted on an upright axis. Representations of animals in various postural sequences were placed inside around the walls of the cylinder and below the slits in such a way that when viewed through the slits and under suitable rotational speeds they were no longer apprehended as independent objects but were seen as momentary phases of a single object in motion. In 1846 Plateau invented his "Anorthoscope," which consisted of two discs, one slightly in front of, and parallel to, the other. The anterior disc was opaque with open radial sectors, and the other, a transparent disc, bore upon it representations of various objects. Both discs rotated about a common axis, which was so controlled as to allow direction and rate of rotation of the posterior disc to vary independently of the one in front. In this manner Plateau produced stroboscopic movement in connection with the figures outlined on the second card. During the following years a large number of devices of various types were built for the purpose of producing apparent movement under successive retinal stimulations. Most of these early devices were not primarily constructed for scientific uses. Thus neither careful control of objective conditions nor accurate description of the movement-experience appears from their employment. Furthermore, no elaborate explanations are advanced. Most of the investigators rest content with a statement of the immediate physical conditions under which the 'illusory' motion appeared. It was not until Exner became interested in the problem of visual movement that we find a serious attempt at control and analysis.

Exner (9) was early impressed by the apparently simple and irreducible character of visual movement, and he marshalled as proof of its elementary character a large amount of factual material. Along with the fact of the negative after-image, he pointed out that the minimal temporal interval necessary for the apprehension of movement is less than that which is required for the perceived succession of two successive lights. The shortest time for the apprehension of two lights in sequence he found to be about 45 σ. When the two, however, were the beginning and the end of a movement, real or apparent, the temporal interval might fall as 14 σ (10). The fact that an object when moving may be apprehended when the distance travelled has less extent than that which is necessary to perceive as discrete two stationary objects was also demonstrated by Exner. A much more important contribution and one later verified and cited by Wertheimer (49) as testimony to his physiological theory was the obtaining of movement through the rapid successive stimulation of the two eyes separately. This fact, it is alleged, substantiates the theory of central conditions as over against peripheral or retinal. We have thus the important statement from Exner that the immediate neurological correlate of the movement sensation is a specific physiological process which runs its course in a hypothetical movement-center in the central nervous system. Following Exner[2] Vierordt believed that visual movement was purely sensational in character. "Ich habe . . . . die Essentialität der Bewegungsempfindungen nachzuweisen gesucht und werde in den nachfolgenden Erörterungen den

[2]For a general discussion of Exner's position in regard to visual movement, see Lasersohn (27) and Wohlgemuth (53).

Beweis anstreben, dass die Raumsinnsorgane (Cutis und Auge) uns von den bewegten Objekten immer nur Bewegungsempfindungen verschaffen" (47, 227). Aubert also could find no other way out. He differed, however, from Exner in his emphasis upon peripheral conditions. "Die Bestimmung, welcher Raum auf der Netzhaut in 1 Sek. durchlaufen werden muss, wenn das Objekt bewegt erscheint . . . . gestattet nun weiter eine Berechnung der Zeit, welche mindestens zwischen zwei Erregungen verschiedener Netzhautelemente vorgehen muss, damit wir eine Bewegung empfinden. Bei einer Winkelgeschwindigkeit von 1′ ist der in einer Sekunde zurückgelegte Weg auf der Netzhaut = 0.00436 mm = 4.36 μ, und da der Durchmesser eines Zapfens in der Fovea, 0.6 μ (= 10″) beträgt, so würden in 1 Sek. etwa 7 Zapfenspitzen in Zeitintervallen von 1/7 Sek. erregt werden. In früheren Beobachtungen hatte ich die Grösse eines Empfindungskreises der Fovea zu etwa 45 Sek. bestimmt: es würden also innerhalb einer Sekunde etwas mehr, als ein Empfindungskreis passirt werden müssen, um die Empfindung einer Bewegung auszulösen, in dieser Zeit aber eine Anzahl von 7 Zapfenspitzen in unmittelbarer Aufeinanderfolge erregt werden. Die Zeit von 1/7 Sekunde für ein anatomisches Netzhautelement, von etwa 2/3 bis 1 Sekunde für einen Empfindungskreis der Netzhaut ist nach anderweitigen Erfahrungen zu gross, um eine Verschmelzung der Bilder, zu klein, um ein vollständige Discontinuität der einzelnen Erregungen eintreten zu lassen—es bleibt eine Beziehung zwischen dem zur Zeit wirkenden und dem 1/7 Sek. vorher dagewesenen Reizzustand bestehen, und der Ausdruck dieser Beziehung ist eine Empfindung *sui generis*, die *Bewegungsempfindung*" (2, 357-358). In the same year with Aubert, Fischer (11) published.[3] The appearance of this article precipitated a discussion concerning the true character of the phenomenon and its correct explanation which has been continued to the present time, although with much less emphasis in recent years. Fischer, believing the after-image essential to stroboscopic movement, states that, "um die Bewegung eines Gegenstandes künstlich nachzuahmen, ist nothwendig und hinreichend, dass man in kurzer Aufeinanderfolge von verschiedenen, nahezu in gleichen, nicht zu grössen Abständen liegenden Phasen der Bewegung desselben kurze Lichteindrücke bekommt, und dass das Auge in der Zwischenzeit durch keine anderen Lichtreize afficirt wird" (11, 149). He thought he proved that there was an optimal time for the clearest apprehension of the apparent movement of an object from one position to another; but he pointed out that, once movement was seen, it tended to be perceived under conditions which would otherwise give no movement. When the time approaches the minimal or maximal, due to an increase or decrease in rotational rate the movement obtained under optimal time disappears. Here we find (1886) one of the earliest statements concerning the relation between the optimal, minimal and maximal temporal intervals and the experience of visual movement.

A clever experiment in visual movement and its dependence upon the number of phases and the temporal interval between them was conducted by Grützner (12). In character it differs little from some of the more recent investigations. His demonstration that "das Auge etwas sieht, was es strenge genommen, nicht sehen kann" was very simple. He covered certain phases of a serial and stroboscopic representation of one boy leaping over the back of another, so that the observer had given only the "Absprung und den Niedersprung" yet one clearly saw that "der eine Knabe über den anderen hinüberfliegt" (p. 519). "Man sieht ihn auf das deutlichste hinüberfliegen. Das geistige Auge ergänzt also eine Bewegung, von der das leibliche Auge nur den Anfang und das Ende sieht, zu einer vollkommenen Bewegung." Grützner's results were carefully investigated by Dürr (8), who found that the movement-experience might be gradually

[3] Fischer used a modification of Stampfer's and Horner's instruments.

destroyed by progressive elimination of successive phasal postures, so long as no corresponding change in rate of presentation was made; that is to say, so long as the time between the objects remained the same. By decreasing the temporal interval he was able to remove many more intervening phases than was possible under a slower sequence.[4] Prior to Dürr's study, Marbe had published his results on the "stroboskopischen Erscheinungen" (**34**). Here we are told that "Auf grund unseres Experimentes mit den Bildern des Reiters bin ich nun zur Ansicht gelangt, dass der Ausfall[5] von Bewegungsphasen in Folge centraler Ursachen unbemerkt bleibt. Das Experiment mit dem Reiter gab uns ein Beispiel dafür, dass eine Reihe von Bewegungsphasen unter Umständen ohne Nachtheil ausfallen kann" (**34**, 399). In Marbe and Dürr, then, we find a partial return to the physiology of the central nervous system as an explanatory basis for the visual apprehension of movement. Only 'partial', however, inasmuch as ocular activities also play a part in their theories; namely, muscular functions, *Anklingen* and *Abklingen* of retinal processes, and the fusion of retinal excitations. An analogous case may be cited in the visual quality objectively conditioned by the rapid rotation of a disc composed of two differently colored sectors.[6] It is well known, Dürr says, "dass successiv und periodisch wirkende Gesichtsreize unter Umständen eine constante Empfindung erzeugen können" (**8**, 501). From Marbe we have the statement that the stroboscopic phenomenon arises when the eye is presented with successive single phases, which are integrated under the influence of the central nervous system. Linke observes that "Marbe hält die 'Verschmelzung' für die eigentliche Grundbedingung, eine *conditio sine qua non*, der stroboskopischen Erscheinungen" (**31**, 450). In the work of Stern (**44**) we find a recognition of the perceptual nature of the movement-experience which depends, as he thinks, upon three factors: modification in stimulus, the flow of after-images, and eye-movements. All appear to be necessary for the perception of visual movement; but they may exist in different patterns. Stern refers to the perception of a particular movement which closely resembles the '*gamma*-movement' named and experimentally investigated much later by Kenkel. "Jede Erhellung erscheint als eine Vergrösserung, jede Verdunkelung als eine Verkleinerung des erleuchteten Objektes. Freilich gehen ja Gestalts- und Helligkeitsveränderungen oft Hand und Hand; aber selbst da, wo jede objektive Grössenveränderung des hellen Gegenstandes absolut ausgeschlossen ist, hat man oft den unverkennbaren Eindruck, als 'rühre sich etwas im Sehfeld' " (**44**, 329).

In the experiments of Linke we find a series of investigations which have considerable historical significance for the *phi-phenomenon*. Linke successively presented (by means of a modification of Horner's stroboscope) two opposed right angles in such a fashion that two vertical lines, for example, coincided and two horizontal lines extended in opposite directions. Under these conditions, together with a proper rate of presentation, there was an obvious turning[7] movement of the horizontal lines around the vertical 'axis'. He also presented a vertical line followed by a horizontal with the result of an "Umklappen" of the vertical toward the horizontal

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[4] "Ein ziemlich grosser Abschnitt einer stroboskopischen Bewegung kann ausfallen, ohne dass die Unterbrechung bemerkt wird, wenn nur die Dauer der Unterbrechung eine hinreichend kurze ist" (p. 512), a fact to be attributed to the "Dauer der Netzhauterregung und der Thatsache der Augenbewegungen."

[5] "Die stroboskopische Bewegungserscheinungen beruhen ausserdem auf der Tatsache dass wir in Folge rein centraler Verhältnisse den Ausfall von Bewegungsphasen nicht bemerken."

[6] "Diejenigen stroboskopischem Erscheinungen bei welchen es sich nicht um das Sehen bewegter Bilder handelt, beruhen im wesentlichen lediglich auf den Thatsachen des, 'Talbotschen Gesetzes' " (**34**, 398).

[7] "Am meisten dürfte aber folgendes für die Beteiligung zentraler Faktoren sprechen: die Richtung der Drehung wechselte in einer von objektiven Bedingungen gänzlich unabhängigen Weise: oft sogar während eines und desselben Versuches. Die 'Windmühlenflügel' schienen sich bald im Sinne des Uhrzeigers zu drehen, bald entgegengesetzt" (**29**, 217).

(31, 526). "Diese Drehbewegung wurde aber nicht erschlossen, sondern—das ist das Wichtigste—unmittelbar 'gesehen'. Dieses 'Sehen' eines objektiv nicht gegebenen Tatbestandes wird sich am ungezwungensten als Assimilations-prozess auffassen lassen . . . . Waren überhaupt nur zwei Phasen geboten, zwei aufeinander senkrechte Kreisdurchmesser, abwechselnd als liegendes und als stehendes Kreuz, auch hier sah man Bewegung. Die Durchmesser schienen wie Windmühlenflügel um ihren Schnittpunkt zu rotieren" (29, 217). Linke stresses central conditions, since attention and identification must, of necessity, be matters resting primarily upon cortical, rather than peripheral, functions. His position is made clear in the following quotation. In order for a movement to be apprehended "ist zunächst nötig, dass mindestens zwei Gesichtswahrnehmungen nacheinander bestehen, die in ihren räumlichen Bestimmungen wenig genug voneinander abweichen, um identifiziert, d. h. auf einen einzigen Gegenstand bezogen werden zu können. Zweitens aber muss diese Identität oder Einheit unmittelbar erlebt werden, und dazu ist nötig, dass die beiden fraglichen Wahrnehmungen rasch genug aufeinander folgen, um als ein einziges, einheitliches Ganze im Bewusstsein zu wirken" (31, 544). Unable to agree that visual movement is either a sensation or a perception, Stratton (45, 46) discovers in the experience something that is neither. "The perception of a motion seems to be nothing more or less than the perception that a sensation is changing its space relations, the motion itself furnishing a decidedly favorable, but by no means unique, set of conditions for appreciating such differences of space relations. This does not imply . . . . a deliberate comparison of positions; for the discrimination often occurs at a single psychic stroke," which, however, is very complex. There is a "relational activity of the mind which interprets and gives character . . . . to the incoming sensations, so that they . . . . mean movement" (46, 443). It appears that "mind becomes accustomed to this manner of interpreting all rapid change" (referring to motion); "it interprets it instinctively as motion" (45, 288). "Where no physical 'thing' is caught" we might call the consciousness of motion, he tells us, "infra-percepts or aesthamorphs" (45, 292). Visual movement lies between sensation and perception, for "consciousness of motion or rapid succession is not a mere sensation, nor is it a mere judgment, nor again is it a perception. . . . It is rather a sensation or a group of sensations consciously organized and yet not organized into anything so concrete as a thing or substantial object" (45, 293). Stratton's labored phrasing here reminds us of Wertheimer's contention that "Es ergab sich in vielfacher Beobachtung immer wieder dieses Charakteristische: klare, zwingende Bewegung um 90° war da, das spezifische 'Hinüber' ist klar zu beobachten; . . . . nicht als Hinüber des Streifens; bloss ein 'Hinüber', eine 'Drehung' " (49, 224), and again of Kenkel's statement that "hat der Beobachter einen deutlichen Bewegungseindruck, ohne ein sich bewegendes Objekt zu erblicken, so nennt man dies reine Bewegungserscheinung" (16, 362).

Much of the setting for the *Gestalt-Theorie* must be indirectly attributed to Schumann who, in 1902, studied the apprehension of movement in connection with the Müller-Lyer illusion. Long before Wertheimer, Schumann, presenting simple geometrical figures in his tachistoscope, obtained clear movement of the first object from its position over toward the position of the second. In 1907, when referring to Linke's work, he intimates that Linke did not realize that the "interessante Tatsache, dass schon zwei Phasen genügen, um den Bewegungseindruck zu geben, ist nicht neu." "Sehr schön," he adds, "kann man sie konstatieren . . . . wenn man, z. B. zuerst den senkrechten Balken eines Kreuzes für einem Moment exponiert und unmittelbar darauf den horizontalen; dann tritt schön der Eindruck einer Drehung des vertikalen Balkens auf" (42, 218). At times, however, the 'object' seems to 'drop out' and there is only an "Etwas" which "sich hinüber bewegt." By way of explanation Schumann

says "Wenn ich von einem sinnlichen 'Etwas' sprach, das bei stroboskopischen Versuchen zu den Wahrnehmungs-bildern hinzukommt, so habe ich damit nicht einen Bewusstseinsinhalt gemeint, der durch Prozesse in der Netzhaut hervorgerufen wird . . . . . nur ein zentraler Vorgang in Frage kommen kann. Er liess von zwei rasch aufeinanderfolgenden, räumlich benachbarten Gesichtseindrücken jeden nur auf ein Auge einwirken, und doch war der deutliche Eindruck der Bewegung da" (41, 183). Lasersohn, too, reports a relevant experiment carried out in Schumann's laboratory. "Wenn nämlich zuerst ein kleinerer und unmittelbar darauf ein grösserer konzentrischer Kreis exponiert wird, so kann man noch dem inneren Erlebnis dadurch Ausdruck geben, dass man sagt: 'Es schien ein kleinerer Kreis sich zu einem grösseren auszuweiten" (27, 121).

We have tried to indicate the more significent aspects of the various contributions, experimental and non-experimental, to the literature on the experience of visual movement prior to Wertheimer's investigation. We have traced the development of objective conditions from the simple whirling card with its mirrored pictures to the highly complicated apparatus of the modern psychological laboratory. We turn now to a brief consideration of Wertheimer's study.

With the work previously done at Frankfort behind him and with Schumann's experience as a guide, Wertheimer set about his task. He took as his principal objects two lines set parallel or in an angle. Many variations in the size, color and position of these figures were made to determine their effect upon the character of the movement-experience (49, 262-265). The objects were presented to 3 *Os*[8] in a definite temporal sequence, by means of the Schumann tachistoscope. The temporal interval proved—as Wertheimer anticipated—to be of great significance for the determination of a certain type of movement. Concerning the importance of this temporal interval for stroboscopic movement, Wertheimer writes: "Man zeichne . . . zwei Objekte einfacher Art. Bei relativ sehr langsamer Rotation . . . . erscheint zuerst die eine Horizontale, dann die andere; die beiden treten klar sukzessiv und dualiter auf. Bei sehr schneller Rotation sieht man sie simultan übereinander; sie sind gleichzeitig, zusammen da. Bei einer mittleren Geschwindigkeit sieht man bestimmte Bewegung: ein Strich bewegt sich klar und deutlich von einer oberen Lage in eine untere und züruck" (p. 165). The spatial as well as the temporal relations of the two objects appear thus to depend upon the length of the pause between the two exposition periods. "Im extremen Simultanstadium sind sie zusammen gegeben, man sieht einen Winkel. Im Bewegungsstadium, zwischen den bieden Extremstadien, dreht sich eine Linie aus der Schräglage in Horizontallage und umgekehrt" (p. 165). Kenkel has given us a more concise statement. "Wenn ich wenigstens zwei Objekte in richtigem Zeitabstande und in verschiedenen Lagen dem Auge darbiete . . . das erste Objekt bleibt eine Zeitlang sichtbar, aber nicht in Ruhe, sondern geht in die Lage des zweiten über . . . . Sukzessivstadium, in dem beide Gegenstände nacheinander in Ruhe erscheinen, entsteht bei längerer Pause zwischen der Darbietung der beiden Objekte. Ist die Aufeinanderfolge sehr rasch, so erscheinen beide Gegenstände gleichzeitig in Ruhe, höchstens mit etwas Innenbewegung im Objekte selber; es ist dies das Simultanstadium, dazwischen liegt das Optimalstadium, das gute sinnliche Bewegung gibt . . . . Biete ich, z. B., tachistoskopisch zwei Linien in gleicher Lage

[8]The only formal instructions recorded were "Fixieren Sie den Scheitel, aber achten Sie besonders darauf, was an dem Orte des oberen Endes der schrägen Linie da ist, geschieht, vor sich geht, so dass Sie darüber genau berichten können" and "konsentrieren Sie Ihre Aufmerksamkeit dauernd fest an dieser Stelle" (49, 204).

nacheinander dar, die in ihrer Richtung einander decken, aber verscheidener Länge sind, so sehe ich im Optimalstadium nur eine Linie, die länger oder kürzer wird, je nachdem, ob die zweite Linie länger oder kürzer ist" (16, 361ff.). With regard to the exact length of the temporal interval which is said to condition these three stages, simultaneous, optimal and successive, we learn that "Unter den gegebenen Umständen führten die Expositionen in der Regel zu optimalen Bewegungseindrücken bei einer Grösse der Zwischenzeit von ca. 60 σ; ruhige Simultaneität erschein bei $t=30\ \sigma$; ruhige Sukzession in der Gegend von $t=200\ \sigma$" (49, 178). We see, then, that for Wertheimer the pause is of very great significance. It is, then, with surprise that one reads from Hillebrand that optimal movement may be obtained when $t=0$. "Wertheimer meint, das Optimum der stroboskopischen Bewegung finde bei einer gewissen mittleren Grösse der Zwischenpause statt; sei diese zu klein (30 σ) so sehe man die beiden Punkte A und B streng gleichzeitig: sei sie zu gross, so sehe man sie hintereinander, aber ohne vermittelnde Bewegung. Das letztere ist nun richtig und wir haben den Grund bereits erkannt. Das Erstere ist aber durchaus falsch; selbst bei $t=0$ kann man die schönste stroboskopische 'Ganzbewegung' erzielen" (13, 243). Hillebrand is emphatic "dass man selbst ohne Zwischenpause Ganzbewegung erzeugen kann" (p. 244).

We have spoken of 'types' of movement, thus indicating that movement is, for Wertheimer, not always of one sort; that it may assume different forms under different conditions. He describes several specific kinds.[9] In one type, only one of the objects moves.[10] He contends that slight quantitative changes in the temporal sequence may result in a shift of type. These shifts bring out the following forms. Full-movement, in which a figure appears to move all the way from its position to the position occupied by a second object; part-movement, in which one object moves back and forth within a more or less restricted area while the other object remains at rest; dual-movement in which each figure is apprehended in motion over an area limited to the space immediately surrounding it; and finally movement within-a-given figure. The experience of movement between two objects, where the movement appears after the presentation of the second figure but is usually conjoined with the first and where nothing in the object corresponds to the experience of movement, Wertheimer called the *phi*-phenomenon. It may occur either in definite conjunction with a specific object or it may exist independent of any particular 'object' meaning. In this last case it may be spoken of as the pure *phi*-phenomenon.[11]

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[9]Linke claims priority in the descriptive account of these movements. He intimates that Wertheimer has only changed the terminology. "Meinen 'unmittelbaren Bewegungseindruck' nannte Wertheimer das *Phi*-Phänomen, meinen 'stroboskopischen Effekt' das Optimal stadium, meinen 'thaumatropischen Effekt' das Simultan-, meinen 'Ruheeffekt' das Sukzessivstadium" (30, 262).

[10]Wertheimer criticizes all theories which make movement involve both, since one figure may be at rest while another is moving, or both may move independently. Thus movement has to be something introduced, so to speak, into the apprehension of a particular spatial pattern.

[11]"Gegeben sind sukzessiv zwei Objekte als Reize; diese werden empfunden; zuerst wird *a* gesehen, zuletzt *b*; zwischen ihnen war die 'Bewegung von *a* nach *b* gesehen;' ohne dass die entsprechende Bewegung resp. die raum-zeit-kontinuierlichen Zwischenlagen zwischen *a* und *b* wirklich als Reize exponiert gewesen wären. Der psychische Sachverhalt sei—ohne irgendeine Präjudiz—mit *a Phi b* bezeichnet; *Phi* bezeichnet, was ausser der Wahrnehmung von *a* und *b* da ist, was zwischen *a* und *b*, in den Zwischenraum zwischen *a* und *b* vor sich geht; was zu *a* und *b* hinzukommt . . . . *Phi* ist etwas, *a* und *b* einheitlich betreffendes, sich auf ihnen aufbauendes, sie beide fassendes und verbindendes. Der phänomenale Inhalt von *Phi* ist durch subjektive Ergänsung (oder auf Grund subjektiver Ergänsung) der objektiv nicht vorhandenen raum- zeit-kontinuierlichen Zwischenlagen gegeben. Man müsste demnach sagen: *Phi* ist eine Angelegenheit, die schlechthin *a* und *b*, und zwar einheitlich betrifft, einheitlich verbindet; und *a* und *b* müssen als notwendige, als die irgend fundierenden, tragenden Inhalte für *Phi* gedacht werden; endlich: *Phi* entsteht, indem zwischen den Lagen *a* und *b* die Zwischenlagen subjektiv ergänzt werden" (49, 185f). Discussing the pure *phi*-phenomenon, he asks "Was ist psychisch im Bewegungsfelde gegeben? Die . . . . These sagte (p. 221): die Zwischenlagen des Objekts werden subjektiv ergänst . . . . das Wesentliche des Hinübergehens resp. des Drehens nichts mit sub-

We obtain a further clue to this "grin without the cat" when, referring to pure *phi*, Wertheimer says: "Dieses *Phi*-Phänomen . . . . war etliche Male so stark da, dass der Beobachter nichts über die Objekte selbst angeben konnte; 'was für Objekte da waren, ist nicht zu sagen; ich habe eine starke Bewegung gesehen, ... von Objekten weiss ich nichts, weiss nichts von Objekten gesehen zu haben' " (49, 225). In the writings of Koffka, too, we find emphasis laid upon the unitariness of *phi*. Under proper experimental conditions "das Erlebnis der einheitlichen Bewegung ist ebenso gesetzmässig und unvermittelt gebunden wie an die Wirkung eines einzigen Reizes die ihm entsprechende Empfindung. Die einzeln nacheinander dargebotenen Reize sind, ... lediglich Reize für das Bewegungserlebnis, nicht es fundierende Inhalte" (21, 354). Koffka seems here to assume that, where two spatially distinct objects are presented with a temporal interval, the temporal and spatial phases of the peripheral excitatory processes bear a one-to-one relation to the temporal and spatial aspects of the physical object;—"die einzelnen Teile einer objektiv gegebenen Figur stehen zum Erlebnis der Gesamtgestalt in direktem Reizverhältnis." We must remember, however, that the visual field under normal conditions is always a totality. The retina is being constantly stimulated. Any shift in the retinal pattern must, of necessity, involve the whole retinal outspread. The various parts of the retina do not functionate independently; rather do the facts indicate a functional integrity of the whole. Any change in the energy relations at one part will be reflected in all other parts by a change in functional activity.[12] To explain the apprehension of movement where there is no physical movement Wertheimer assumes a "short-circuiting" between two cortical regions. The peripheral conditions are of minor importance; instead it is "bestimmte, zentrale Vorgänge, physiologische 'Querfunktionen' besonderer Art, die als das physiologische Korrelat der *Phi*-phänomene dienen." More specifically stated "Wird die Stelle *a* gereizt in bestimmt kurzer Zeit nachher die nahe Stelle *b*, so träte eine Art physiologischen Kurzschlusses von *a* nach *b* ein: in dem Abstand zwischen beiden Stellen finde ein spezifisches Hinüber von Erregung statt; ist der Grad der Umkreiswirkung von *a* z. B. an dem Höhepunkt seiner zeitlichen Verlaufskurve angelangt und bietet sich nun Umkreiswirkung von *b*, so flute Erregung hinüber, ein physiologisch spezifischer Vorgang, dessen Richtung dadurch gegeben ist, dass *a* und die Umkreiswirkung um *a* zuerst da ist. Je näher die beiden Stellen *a*, *b* einander sind, desto günstiger die Bedingungen für ein Entstehen des *Phi*-Vorgangs. Ist *t*, die Zwischenzeit zwischen dem Eintritt der Erregung in die beiden sukzessiv gereizten Stellen *a* und *b*, zu gross, so ist die Umkreiswirkung um *a* schon erloschen, wenn die von *b* eintritt; ist die Zwischenzeit kürzer, so, das die Umkreiswirkung von *a* da ist, ev. am Höhepunkt ihrer Verlaufskurve sich befindet, wenn die von *b* eintritt, so ergibt sich das Hinüber von Erregung; ist *t* sehr kurz, so treten die Umkreiswirkung von *a* und *b* zu gleichzeitig auf, um den gerichteten Kurzschluss zu ermöglichen" (49, 247 f).

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jektiven Zwischenlagen zu tun hat: Es gibt Fälle, wo *Phi*, die Hinüberbewegung, die Drehung, klar und deutlich gegeben ist, ohne dass im Bewegungsfelde der Striche irgendwie vorhanden war; die Anfangs- und die Endlage war da, zwischen ihnen die Bewegung, aber im Bewegungsfelde nicht optische Ergänsung, Sehen oder Vorstellen der Zwischenlagen des Striches. Bei allen Beobachtern ergab sich dieses spontan; in Versuchsanordnung konnte dieses 'reine' *Phi* ohne Ergänsung von Zwischenlagen in einfacher Weise demonstriert werden" (p. 222). "Diese Fälle zeigten sich so, dass auch nicht etwa der Gedanke vorhanden war: ein Objekt habe sich hinüberbewegt; was von Objekten vorhanden war, war in den zwei Lagen gegeben; nicht eines oder eines von ihnen oder ein ähnliches betraf die Bewegung; sondern zwischen ihnen war Bewegung gegeben; nicht eine Objektbewegung . . . es war einfach Bewegung da; nicht auf ein Objekt bezüglich . . . in diesem Felde, wo nichts von den sonstigen optischen Qualitäten zu sehen war (ausser dem blanken Grunde)" (p. 223).

[12]It is claimed that the "subjective" visual field is never homogeneous even under conditions where the objective conditions are everywhere the same (22).

Hillebrand comments caustically upon Wertheimer's cortical theory. "An den Namen Wertheimers knüpft sich eine pseudo-physiologische Theorie (man nennt sie 'Kurzschlusstheorie'), die sich schon als unhaltbar erweist, wenn man ihre verschwommene Fassung durch eine klare ersetzen will. Sie ist tatsächlich nichts anderes als eine Übersetzung des zu Erklärenden in eine noch dazu mangelhafte physiologische Terminologie." A little later he speaks of "hypothetische Vorgänge, die *ad hoc* konstruiert werden, wie dies bei der Kurzschlusstheorie der Fall ist" (**13**, 212f). Watt (**48**) is only less severe. As he sees it Wertheimer formulates a physiological hypothesis which "makes no contribution to psychological theory" (p. 26). Koffka, in complete sympathy with Wertheimer, contends that "We must abandon all attempts to explain it [movement] by one of the customary psychological theories, completion or elaboration of sensations by the so-called higher functions" (**22**, 269).

We see, then, that, correctly considered in its proper historical setting, the work of Wertheimer was not so startling as one might gather from the apparent impression made upon certain psychologists.[13] The descriptive facts were not wholly new; neither was the insistence upon the unity of experience. It is, then, to his explanatory concept that we must attribute, as also does G. E. Müller (**36**, 105), most of his influence. The probem had been previously studied by many individuals throughout years of research upon the movement-experience. There were many contradictory theories; some stressing peripheral, others central, conditions. Exner's demonstration that the central conditions appeared to play a very important role must, as we know, have left its mark upon those who knew their literature.

With this rich historical background Wertheimer approached the problem of explanation. His speculative cerebral explanation has, to be sure, its attractive features. It is novel, and it is partially adequate to the facts. Furthermore, it aids one in thinking, inasmuch as the simplicity of the theory conduces to an easy transliteration into visual terms of unknown physiological events. Let us indicate specifically what appear to be the essential points in the explanation of movement in the doctrine of the *Gestalt*. Assume point *A* in the cortex, stimulated by an excitation from receptor organs; and adjacent to it point *B*, likewise stimulated, either simultaneously or successively. Now, with these two points given, let us assume further that the cortical process arising from the stimulation of point *A* or *B* is not confined or limited to the particular area determined by the stimulus but that it radiates from the original point of arousal in all directions, becoming as is said a spatially and temporally extended process. Streaming out, not unlike rays from a luminous body, or waves from a central disturbance certain portions of the neural process, provided the intensity of the stimulus is of a given degree, pass completely from *A* to the "region" *B*. If the interspatial cortical distance is traversed by the excitation from *A* without intercepting a similar process from point *B*, we have the so-called successive stage. *A* runs its course before *B* appears. But if we assume that *B* is stimulated after a temporal interval such that

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[13]Koffka finds that Wertheimer's contribution presents three outstanding phases; "(1) Descriptive: Diese Gestalten sind in keiner Weise weniger unmittelbar als ihre Teile, 'oft wird das Ganze erfasst, bevor überhaupt die einzelnen Teile ins Bewusstsein kommen.' Die reine Deskription der Erlebnisse kann daher nicht mehr am Empfindungsbegriff orientiert werden, sie wird von der Gestalt und deren Eigenschaften auszugehen haben; (2) Functional: Die typische Form der verbindung Reiz-Erlebnis ist nicht mehr die Empfindung. So wie die Gestalten deskriptiv nicht weniger unmittelbar sind als ihre Teile, so sind sie, funktionell, auch nicht weniger ursprünglich. Der Versuch, das Ganze aus seinen Teilen abzuleiten oder es über ihnen aufzubauen ist sehr oft verfehlt, das Ganze ist nicht durch Zusammensetzung der Stücke entstanden, sondern als der dem Reizbestand gerade korrelate Erlebnisbestand ebenso direkt aufzufassen, wie dies bisher nur bei den Empfindungen üblich war: And finally (3) Physiological: Die typische Form des dem Erlebnis korrelaten Hirnvorgangs ist nicht mehr die Einzelerregung einer Hirnstelle plus Assoziation, sondern es sind Gesamtprozesse und in erster Linie deren Ganz-(nicht Summen-) Eigenschaften, die für die weitere Hypothesenbildung benutzt werden müssen" (**23**, 58 ff.).

the excitation from *A* has not completed its course to *B*, then the two processes will meet provided the interspatial cortical distance *AB* is not too great. Assuming simultaneous arousal and similar rates of outspread for the two, the point of juncture will be half way between the two regions and we will have experientially the stage of simultaneity and neurologically the simultaneous *phi*-function. We may represent this event in this simple way: *A*——m——B. When the two excitations meet at m (half way) we apprehend the figures simultaneously and at rest with no movement between them. Assuming, however, that the cortical process from *A* has just reached m when point *B* is stimulated, then with equal rates of radiation, the meeting point may be represented by O where O stands half way between *m* and *B*, or ¾ the cortical distance from *A* to *B*. We represent it thus: A——m——O——B. Here we have represented the physiology of the optimal stage. In all cases the rate of outspread is determined by the intensity of the cortical excitation which in turn depends upon the intensity of the objective conditions. If the objects are too widely separated, the two excitations will be unable to form a unitary whole. So there will be no movement. If the pause is too long, the same result will follow. And, finally, even though the objects are fairly close together and the pause short, there may be no movement-experience because the intensity is too weak to effect a short circuit. Thus we have three sets of physical conditions; *viz.*, intensity of the stimulus, interstimulus distance, and length of pause to which correspond, on the physiological side, intensity of excitation, rate of flow, and distance of flow. The significance of the pause for the movement phenomenon now becomes clear. During the temporal interval the process of short-circuiting is complete, thus furnishing the neurological basis for movement. During the pause the rate of neural outspread is assumed to be four times as great as during the period of exposition and stimulation.[14] In the latter period, the radiation is assumed to be relatively inhibited. Toward this alleged central filling-out during the interval Hillebrand is skeptical. Referring to certain of his experiments, he says, "Der Hauptsache nach kann man also sagen, dass die stroboskopische Bewegung vom zweiten Punkt gemacht wird und es wird schon dadurch der Gedanke nahe gelegt, dass es sich nicht um eine irgendwie geartete zentrale Ausfüllung der Strecke während der objektiven Dunkelpause handelt (wie dies z. B. die Kurzschlusstheorie annimmt), sondern dass es das zweite Objekt ist, welches während seiner Sichtbarkeit eine lokale Umwertung erfährt" (**13**, 233).

Taking Wertheimer's work as a guide, Kenkel (**16**) investigated the movement-experience as given under conditions where the objects were of the so-called illusory class. His principal figures (pp. 448-449) were various forms of the Müller-Lyer illusion, also circles, sectors and triangles. From the successive presentation of these Kenkel obtained three district types of movement, namely the *Alpha*, the *Beta* and the *Gamma*. The *Alpha*-movement is obtainable only under illusory conditions, inasmuch as it is the movement which comes from the successive apprehension of two lines which are physically alike but appear as spatially dissimilar. Thus the long line in the Müller-Lyer appears to contract or to expand depending upon the order of presentation. The *Beta*-movement is the common type obtained from objects which differ in spatial dimensions or in spatial location. The wings of the Müller-Lyer *e.g.*, fly around giving pronounced

[14]Wittman points out a curious fact which appears, under certain conditions, to be connected with the temporal interval between the two expositions. For a practised *O*, he says, "tritt die Dunkelpause immer mehr zurück, das soeben exponiert gewesene Bild besitzt eine noch erhöhte psychische Präsenz bei Eintritt des neuen Bildes, so dass der Eindruck entsteht, als beginne die 'Bewegung' schon während der Exposition des ersten Bildes und dauere während der Dunkelpause an; dieser Eindruck wird noch verstärkt bei Verkürzung der Dunkelpause, wenn sich die Bewegung dem sog. Optimalstadium nähert; jetzt ist die Präsenz zweier Bilder bei gänzlichem Uebersehen der Dunkelpausen noch wesentlich erhöht'. (**52**, 23).

Beta-movement. To this general class belong Wertheimer's various types of movement. The *Gamma*-movement[15] is that unique experience which appears under short and rapid expositions together with continued observation of a single object or figure. The object appears to contract and expand, particularly under repeated and continuous observation. It is to be noted that the *Gamma*-movement possesses peculiar significance for certain advocates of the *Gestalt-Theorie*. It affords, as it is claimed, indubitable proof of the validity of certain of their hypotheses. How is it possible to apprehend a single unmoving object as in motion? As expanding under illumination and contracting under reduction of the light? The expansive movement is said to be conditioned through the radiation or outspread of the neural excitation from the given stimulated cortical region. When the illumination is quite intense the phenomenal increase is proportionally great, and conversely. The contraction of the figure, on the other hand, is due to the flowing in upon itself of the cortical excitation. Koffka says: "Die Ausdehnung (*Gamma*-Movement) haben wir schon auf die Erregungsausbreitung zurückgeführt, entsprechend deuten wir das Zusammenziehen als Zurückfliessen der ausgestrahlten Erregung in ihren Ursprungsort" (**20**, 270). More recently the *Gamma*-movement has been made the object of an extended investigation by Lindemann (**28**), who has sought in particular the relative significance of the length of exposition time, temporal interval, intensity of stimulus, practise and the particular configuration of the object. He points out the curious fact that when the figure is apprehended as an object (or thing) it tends to remain at rest, but when it is apprehended as a geometrical oval or surface pronounced extension and contraction may be observed. Wingender's (**51**) principal objects were the Hering, Zöllner, and Müller-Lyer illusions. He reports upon the unitariness of movement, and especially upon the fact that the movement is apprehended as temporally disjoined from the form of the objects. "Die Deformationen der Hauptlinie sind beim Hinzuschliessen der Nebenlinien als Bewegung sichtbar: wenn die Nebenlinien schon an ihrem Platz stillstehen, bewegen sich die Hauptlinien mitunter noch weiter" (p. 24).

The discovery of still another type of movement was left to Korte (**26**), who found that under certain conditions of attention and fixation, together with a proper relationship between the intensity of the illumination, the magnitude of the objects, distance between the objects and length of pause, movement could be apprehended with its direction from b to a although a appeared first in the visual field. He calls this the *Delta*-movement and indicates a great number of conditions under which it may be apprehended and the various factors which affect it. Attention and point of fixation he found to be very important. Under a shift in attention in the visual field *Delta*-movement could be made to disappear and to reappear. He sought to express the relationship obtaining between the three major physical conditions (*viz.*, intensity of objects, spatial separation, and length of pause). Where i stands for intensity, s for the distance between the figures, and p for the temporal interval, we have, according to Korte, the following laws.

s (optimal) varies directly with i
i (") varies inversely with p
p (") varies directly with s.

[15] "Anfangs erschien der Gegenstand in Ruhe, kam als Ganzes und verschwand als Ganzes; bei gesteigerter Geschwindigkeit kam zunächst unbestimmte Unruhe in den Gegenstand, die sich bald zu einem bestimmten Zucken, dann zu einem Stossen, Schlagen, Strecken, Zusammenziehen, Klappen usw. verdeutlichte. Die Bewegungserscheinung steigerte sich also mit der grösseren Geschwindigkeit. So verhielt es sich innerhalb der hier gezogenen Grenzen, doch gibt es eine Geschwindigkeitsgrenze, bei der ein Maximum von Bewegung auftritt. Darüber wird der zweite dieser Beiträge berichten. Zuerst verbindet sich die Bewegung mit dem Erscheinen und Verschwinden der Figur, beherrscht aber bei maximaler Geschwindigkeit, die sich mitunter bis zu 8 σ Exposition steigerte, die ganze Erscheinung, wenn auch Erscheinen und Verschwinden noch oft stärker bewegt sind, und zwar ist im allgemeinen das Erscheinen mit Vergrösserung und das Verschwinden mit Verkleinerung verknüpft" (**16**, 402).

The temporal, spatial and intensive characteristics of the objects as so determined were further investigated by Cermak (6), whose results tend to show that the laws have a wider applicability and a greater significance for the *Gestalt-Theorie* than Korte indicated.

In 1920 experiments upon the *phi-phenomenon* (*Beta*-movement) were reported from the psychological laboratory of Cornell University. Under a 'process' instruction the mental correlate of Wertheimer's experience was discovered to be a 'flash of gray'. In his final description of this gray, Dimmick (7) writes: "It must not for a moment be assumed that it is a simple text-book gray, the gray that lies at the center of the color-pyramid, or the gray of a piece of gray paper. The temporal attribute is beaten up with the quality into an integration of the first order. All *Os* describe the experience as a 'flashing,' 'live' gray, ... a 'curtain' or 'film' " (pp. 331-332). Under a perceptual attitude, created by a 'meaning'-instruction, the *Os* apprehended various degrees of movement. There appeared full-movement, including the pure *phi*-phenomenon, bimembral, and unimembral movements, as well as the intramembral variety. On the whole, Dimmick's investigation tended to confirm Wertheimer's descriptive facts. Thus all of Wertheimer's movement-types were reported from the Cornell laboratory. Dimmick further agrees with Wertheimer that there is a stage approaching simultaneity (ca. 30 σ) an optimal stage (ca. 90 σ) and a stage of succession (ca. 150 σ). The investigations differed with respect to the interval necessary for full-movement, the instruction and the report, and the qualitative filling in the field of movement.[16]

In a still more recent investigation of visual movement, in connection with an analytical study of the Müller-Lyer illusion, we find emphasis placed upon the simple and elementary character of the movement-experience. Referring to the nature of visual movement, Bates (3) says that movement is actually "seen in sensory terms," and is "a definite experience distinguished from displacement and change in extent—a sheer experience of the order of mental process" (p. 60). A critical and analytical study has lately been made by Wittman who claims that, although Wertheimer really regards the sensory character of movement as simple and irreducible (when he refers to the "sinnlich klar and deutlich gegeben Eindruck der Bewegung"), his optimal movement is never sensory; but only the "Phasen sowie das Feld in dem sie erscheinen" (52, p. 15). According to Wittman, even under these conditions which give pure *phi* there is still objective meaning (p. 16). Concerning the alleged lack of any spatial or qualitative characteristics in Wertheimer's phenomenon, Wittman has to say of the first that the apprehension of movement between two things "als solche ist[es] zunächst ein Raumerlebnis" and about the second he remarks that of movement "als ein selbständiges Phänomen, das ohne Bezug auf ein Objekt gar mit sinnlicher Qualität erlebt würde, dem als physiologisches Korrelat ein physiologischer Kurzschluss zugeordnet werden könnte, darf er nicht bezeichnet werden" (p. 23). Wittman's own explanation is quite simple. "Mit dem Bewusstsein der Lagenverschiebung verbindet sich sodann das Bewusstsein des Lagenüberganges und das Erlebnis dieses 'Lagenüberganges' is mit dem Sehen einer Bewegung identisch" (p. 22).

We have come now to the end of our historical survey. Although much ground has been covered, it represents, after all, but a relatively small part of all the work done on the visual apprehension of movement. We have attempted to review here merely the outstanding experimental investigations of a

[16]Whitehurch has recently investigated the cutaneous apprehension of movement in the Cornell laboratories. She concludes that "movement is, after all, a gross meaning" (50, 489). *Cf.* H. Burtt, Tactual illusion of movement, *Jour. Exp. Psych.*, 2, 1917, 62-75; 371-385.

certain class of visual movements and to indicate the nature of the contributions, historical, descriptive and explanatory, to the general problem. We have sought especially to trace the antecedents of the *phi-phenomenon*, the phenomenological treatment of which has directly led, as we have indicated, to the origin of the *Gestalt-Theorie* and indirectly, by way of the development of this doctrine, to many important studies in perception. We now turn to a consideration of the problem of the present investigation.

B. *Statement of the Problem.* We are not concerned with the visual apprehension of movement at large and still less do we attempt a general survey of all forms and varieties of this mode of perception. Confining ourselves to vision, we disregard all those perceptions which rest upon an actual motion in the object as well as certain experiences in which no physical object acts immediately upon the visual receptors. Thus we omit the apparent movements due to prolonged fixation, the movements incident to vertigo, illusory dislocations of movement, movement due to the externally turned eye-ball, and the like. All these forms of visual perception involve the organism in a manner unlike that by which successive, resting, retinal stimulations condition the apprehension of movement. It is perceived movement under these last-named conditions that is to be considered in the following pages.

Through the employment of an apparatus devised, perfected and tested throughout several preliminary months an attempt has been made to approach the fundamental problems involved in the studies already reviewed. *Our* problems are four; (1) to discover the significance of the temporal interval, (2) to write a descriptive account of the several aspects of the movement-experience, (3) to scrutinize the explanatory hypotheses of the *Gestalt-Theorie* (so far as these hypotheses relate to visual movement), and (4) to examine the claim that the psychological correlate of visual movement is a certain "gray, fairly brief in duration." As to method, we have tried so to modify objective conditions as to give us a greater control over the extra-organic factors than has been obtained in most of the previous investigations. To this end we have eliminated from the visual field everything except that which was absolutely necessary to the perception.[17]

[17]Improvement in technique is always possible after the work: A semi-transparent membrane over the slits in the stimulus-cards would have reduced irradiation of the light as it passed through the lines.

II. The Experiments

A. *Apparatus.* Our exposure-apparatus was a mirror tachistoscope of the Dodge type. We embodied in it, for our special needs, several modifications, as shown in Fig. 1.

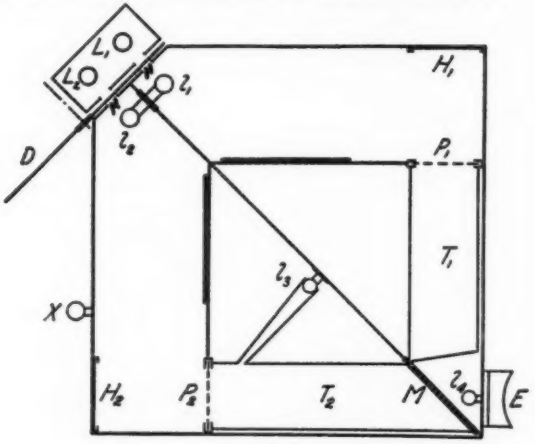

Figure 1.

At P_1 and P_2 frames were constructed to hold the cards carrying the stimulus patterns. At M was placed an adjustable platinum-iridium mirror so made as to reflect at E (the eyes) as much light from P_1 as it transmitted when illuminated from behind, at P_2. Anchored to the bottom of the box and connecting the frames at P_1 and P_2 with the mirror (M) were 4 in. black tubes (T_1 and T_2).

A small light l_3 attached to the diagonal wall was joined by a ½ in. tube to an aperture in T_2. Thus a weak light (too weak for after images) could be thrown upon the anterior side of the card at P_2 for fixation. Another light of variable intensity (l_4) directly over *O*'s eyes reduced flicker. Two moderate lights (l_1 and l_2), controlled independently by a series of 5 keys, presented either object for any desired duration and intensity. Two large, variable 150-watt Mazda lights (L_1 and L_2), housed in a lightproof box in front of the windows (N) of the tachistoscope served as primary source-lights in Series I-IV and VIII.

Exposition times and temporal intervals were controlled by means of the 3-ply wooden disc D (diam. 88 cm.), supplied with two open radial sectors, rotating between the light box and N. One of the sectors was adjustable over a graduated arc of 110°. Before N was placed a card with openings identical with those in the disc. The rate of rotation, and accordingly the temporal interval, was widely and accurately varied by a reduction cone of 16 grooves. Additional variation was obtained by use of a 10 speed d. c. motor to drive the cone. Constant check upon speed was made during experimentation by means of a voltmeter across the terminals of the motor.

B. *Method.* Our general procedure was to present 2 straight lines similar to the principle objects used by Wertheimer and Dimmick. These objects were slits in dark cards (with certain exceptions noted later) placed at P_1 and P_2. They were apprehended under experimental conditions as bright lines upon a dark or lightless field. We shall call the oblique-horizontal arrangement of the lines ($\angle$) "A", and the parallel-horizontals (=) "B", to designate the two principal object-patterns.

In both A and B the lines were 25 mm. long and 1 mm. wide. In A the angle was 45°. In B the interlinear distance was about 15 mm. The stimulus objects were presented either in sequence, with a definite temporal interval, or else (in some series) simultaneously. 8 series of experiments were conducted with A and 8 with B.[18] Every one of the first four series consisted of 20 observations for every *O*,[19] and they covered the 10 temporal intervals 15, 30, 45, 60, 75, 90, 120, 150, 180, and 225 σ. In Series I-IV and VIII the exposition time was 50 σ. These 10 intervals range, according to Wertheimer and Dimmick, from simultaneity to succession. Every one was presented twice during every period of observation. In this way we were able to check possible variations in report under the same objective conditions during the course of a single sitting. The order of the temporal intervals was determined by a chance distribution to avoid cumulative effect or the occurrence of an observational set which might appear under a sequence in which there was a gradual increase or decrease in the duration of the interval.

In Series I-IV, A and B were presented on alternate periods of observation, B following A in each series. Thus, in Series I and III, the oblique was followed by the horizontal (object A) and the upper by the lower horizontal (object B). In Series II and IV the order of presentation of the two lines was reversed. In this way we were able to determine practice effects contingent upon repetition, the effect of one stimulus-pattern upon another, and possible differences in the movement-experience due to a reversal of sequence.

In Series V, a study was made of the visual apprehension of movement under the mere addition of one member to another *without interval*, to form, in the one case, (A), an acute angle and, in the other, (B), two parallel lines. The method was simple. One light (l_1 or l_2) was turned on; *O* attended for 5 sec., and then the other light (l_2 or l_1) was added. Both lines were then simultaneously removed after 5 sec. more. 5 observations were taken for every *O* where the oblique was added to the horizontal, 5 for the reverse order and a similar number for each of the lines in B, making a total of 50 for everyone of the four cases.

Series VI was similar to Series V except that one member was subtracted or removed from the total configuration. Both lines were together presented for 5 sec. and then one line was removed by turning off one of the lights. The same number of observations was made as in V.

In Series VII the temporal interval between the two successive expositions was zero. One object was given for 5 sec. (l_1 or l_2), followed then by

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[18] An additional series of 200 observations was made with stimulus-pattern C-o two adjacently placed circular holes 1 mm in diam. Since there was no difference obtained with this and with patterns A and B, the results are not included.

[19] 10 persons observed throughout the entire experiment. Members of the departmental staff: Dr. C. R. Griffith (G), Dr. P. T. Young (Y), Dr. E. A. Culler (C), Dr. Alice Sullivan (S), Dr. H. L. Kingsley (K), Mr. Donald Rothschild (R), assistant in psychology, and the writer (H); graduate students in psychology: Mr. W. Mikesell (M) and Mr. W. Zuschke (Z), and Mrs. Ethel Higginson (Hn). Mrs. Madison Bentley (B) and Miss Helen Hopkins (Hs), assistant in psychology, observed in Series I-IV. When *E* acted as *O*, Hn took his place at the apparatus.

the other (l_1 or l_2) at an intensity such that the instantaneous appearance of the second object effected the simultaneous disappearance of the first (although the first light was unchanged). In order to obtain the degree of intensity required to wipe out the first figure it was necessary to change the character of our stimulus-objects, since the slitted lines would not transmit sufficient light energy. We accordingly drew the objects in black upon a light grey card and placed them against the outer wall at H_1 and H_2. The slitted cards were removed at P_1 and P_2 leaving a rectangular opening 3x2 in. through which the objects at H were apprehended. Using this arrangement we found it very easy to present one figure for any desired time and then to shift instantaneously to the other again. 5 observations were made by every *O* in every one of the four possible cases.

Series VIII differs widely from all the preceding, where only 1 exposition of each figure was made before *O*'s report. In this last series continuous observation (5 exposures) was made with every temporal interval. 3 reports were made by every *O* for every one of 11 intervals (0-interval added to the 10 above.)

The entire experiment was conducted in a dark room with one small unenclosed light (X). *O*'s seat was completely enclosed by black cloth on three sides and the top. Under these conditions there was complete removal of all stray light.[20]

O sat within the curtained embrasure with the head maintained firmly in one position by an adjustable support. At the signal "Ready" the control key was lifted giving the fixation-light. Immediately following the signal "Now" and just after the sectors in the disc had passed the light-box, the key was pressed down, removing the fixation-light and turning on the large lights (L_1 and L_2).

These lights always had more than ½ sec. in which to attain full brightness before the radial sectors again reached the light box. In this way all effects of inertia in the lights were eliminated. After the openings had again passed N giving one exposition of each member of the object (except in Series VIII), the key was released. During the moment that *O* took to organize the report, the experimenter released the motor, stopped the disc, reset the movable sector or the motor for a different time-interval and restarted the motor. After the report was taken the next exposition was immediately given. This was the general procedure for A and B in Series I-IV and VIII.

All the *O*s worked under formal instruction which was given before every sitting.

"Fixate steadily the small lighted area in the box. Close first the one eye and then the other to make sure that you see the light with both eyes. When the lighted area disappears, continue to observe the field without change in fixation.

Observe carefully whatever comes and report orally to *E*. Be sure to include in your descriptive account particular and precise statements as to the order and nature of the appearance and disappearance of the phenomenal object or objects.

At the end of each report, indicate by the number 1, 2, 3 or 4 the degree of assurance concerning the fullness and accuracy of your report. The numbers 1, 2, 3 and 4 stand as follows: 4 represents a high degree of assurance; 3, moderate assurance; 2, small assurance; and 1, complete or almost complete lack of assurance."

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[20]Every *O* was allowed 2 min. for adaptation; although it was soon found that adaptation had little, if anything, to do with the matters in question.

Our preliminary reports were not sufficiently standardized. The demand for a "descriptive account" proved to be ambiguous, as it obviously as in Wertheimer's experiments. In our regular experiments we specified as follows:

"Do not speculate upon the apparatus or upon experimental conditions. You are not to guess at or to reconstruct the conditions in the apparatus. This is not a test of alertness or of intellectual acuteness. You are to omit all assumptions, inferences or associative reflections, as well as all analogies and outside comparisons. Strive to translate as precisely as possible into verbal terms your actual visual experience. Nothing else. Make it photographic. You are not depicting an event in its natural setting. You are describing a phenomenal, not a natural, object. Do not make your reports figurative. Confine them to qualitative, temporal and spatial properties and relations."

III. The Results

Series I-IV; Object A. The results of this series are summarized, on the quantitative side, in Table I. The upper horizontal line of figures after every *O* gives the total number of each type of movement apprehended in Series I and III (a-b sequence); the lower line, in Series II and IV (b-a sequence). There were always four trials.

The most obvious fact in the Table is the prevalence of cases of full movement (FM); *i.e.*, movement from the position of one line to that of the other. Of 81 cases of "no-movement", all but 26 are from two *O*s (M and Z).[21]

It must not be assumed that 'full-movement' always meant that a line was apprehended to move, as a line, across the field; although this is the usual meaning of the term in previous studies. The *O*s instead reported at times that the experience *meant* that one line moved from one position to another; but that the actual movement of a line across the field intermediate to the two perceived positions was not seen. At other times, it seemed to be just bare movement over the field. Description of this movement was very difficult. It was insistent yet baffling. It was an approximation, apparently, to Wertheimer's 'pure *phi*-movement'. At least this is indicated by such reports as the following:[22]

"Two bright lines; the oblique slightly first. Movement referred to the whole region between the lines but not the movement of a line. (Perhaps a judgment in terms of eye-movements)" (G, 4).

"A horizontal line, broken in the center, appeared in the lower part of the visual field. The line was not sharply defined. Then a clear oblique appeared. The two formed an angle. The horizontal then disappeared and the oblique persisted for a moment. Between the lines and filling the space was a yellow haze of low chroma. I saw very rapid movement from the lower to the upper position. I am sure of movement, but not sure it was a line" (Y, 5).

"A horizontal line appeared and disappeared; then an oblique line appeared. There was no qualitative change in the field between them. There was, however, an impression of movement from one position to another, but not of a line" (Hs, 10).

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[21] M had difficulty in adjusting himself in the beginning to our experimental conditions. He frequently complained that he failed to focus. He likewise observed that the total exposition was so rapid that he was unable clearly to perceive the object. It was often necessary to repeat in order to obtain a report. Z appeared at times to work under the self-instruction that the change was not true movement unless it approximated the nature of a physical object in motion.

[22] The numeral after the letter for the *O* shows the temporal interval from which the report was taken.

TABLE I, Series I-IV; with Object A

	1 15				2 30				3 45				4 60				5 75				6 90				7 120				8 150				9 180				10 225			
	O	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I
G		4				4				4				3			I	4				4				2	2		I	4				4				2	2		
	I	I	2			3	I			3	I			4				4				3	I			I	2	I		3	I			I	I	3		2	2		
Y		4			I	4				4				4				3	I			4				4				4				2	2			3	I		
		2	2			4				4				4				4				3	I			3	I			4				4				4			
C		2	I	I		4	I			4				4			I	3			I	2	2			3	I			4			I	3	I			4			
		2		2		2		2		4				4				4			I	4				4				2	2			2	2			2		2	
S		4				4				4				4				4				4				4				4				4				4			I
		4				4				4				4				4				4				4				4				4				4			
B		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
K	2	2				4				3	I	I		4				I	2		I	4				4				4				2	2			4			
		4				4				4				4				4				4				3	I			2	2		I	4				4			
Hn		4				4				4				4				4				4				4				4				4				4			
		2		2		I		3		I	I	2		3		2		3		2		4		4		2	I	I		2		4		2		4		3		I	
H		4				4				4				4			I	4				4				4				4				4				4			
		4				3	I			4				4				3			I	4				4				4				4				4			
Hs	I	3				4			I	3				2	2			4			I	3				I	3			2	2			4				4			
	I	3				4				4				4				4		I		3		I		4				4				4			I	3			I
R		4				I	3			4				4				3	I			4				I	3			4				I	3			2	2		
		2	2			4				3		I		2	2			I	3	I		3	I			I	3			I	3			4				2	I	I	
M		3	I			I	3			4				4				3	I			4				3	I			4				2	2			3	I		
		4				2	2			4				4				I	3			2	2			2	2			2	2			4				I	3		
Z		3			I	I	2	2				I	I	I	2			4				I	2	I		2			2	3			I			3	I			2			2
	2	2				4			I	2		I	I	3			I		3		I	I	3			4				2	I	I		4				2		2	

O=no movement; F=full; P=partial; D=dual; and I=inner movement. Full movement followed by a partial return is entered as FP; followed by a displacement of the end-position is entered as FD.

TABLE II, Series I-IV; with Object B

	1				2				3				4				5				6				7				8				9				10			
	O	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I	F	P	D	I
G		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
Y		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
C		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
S		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
B		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
K		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
Hn		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
H		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
Hs		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
R		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
M		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			
Z		4				4				4				4				4				4				4				4				4				4			
		4				4				4				4				4				4				4				4				4				4			

TABLE II, Series I-IV; with Object B

	1 15 O F P D I	2 30 O F P D I	3 45 O F P D I	4 60 O F P D I	5 75 O F P D I	6 90 O F P D I	7 120 O F P D I	8 150 O F P D I	9 180 O F P D I	10 225 O F P D I
G	4 1	2 2	3 1	4	1 3	4	3 1	4	1 3	2 2
	4	4	3 1	2 2	1 2 1	2 2	3 1	4	2 2	2 2
Y	4	4	4	4	4	4 1	4	4	4	1 3
	4	3 1	1 3	2 2	4	1 3	1 3	3 1	2 2	2 2
S	4	4	4	4	4	4	4	4	4	4
	4	4	4	4	4	4	4	4	4	4
C	2 2	4 1	4	3 1 1	3 1	2 2 1	2 2	2 1 1	4	4 1
	4	4	4	4	4	4	4 1	3 1	4	4
Hn	4	4	4	4	4 1	4	4	4 1	4	4 2
	4	4	4	4	4	4	4 1	3 1	4	3 2
Hs	4	4	4	4	4	2 2	3 1	1 2 1	1 3	1 3
	4	4	4	4	4	4	3 1	4	4	2 2
M	2 2	2 2	4	1 3	1 3	4	4	1 2 1	3 1	3 1
	1 3	2 2	4	1 3	4	4	2 2	4	1 2 1	4
B	4	4 1	4	4	4	4	4	4	4	4
	4	4	4	4	4	4	4	4	4	4
Z	3 1	1 3	2 1 1	2 2	4	1 2 1	2 1 1	3 1	4	3 1
	2 2	4	3 1	2 2	4	3 1	2 2	1 3	2 1 1	1 3
H	4	3 1	4	4	4	2 2	4	4	4	4
	4	3	4	4	4	4	4	3 1	4	4
K	4	1 3 1	4	4	4	4	4	4	4	2 2
	2 2	4	2 2	4	4	4	4	4	4 1	4
R	1 3	4	4	4	4	4	4	4	4	4
	3 3	4 1	4	4	4	4	4	4	3 1	1 3

TOTALS	15		30		45		60		75		90		120		150		180		225	
Tables	I	II	I	II	I	II	I	II	I	II	I	I	I	II	I	II	I	II	I	II
O	12	8	6	5	3	5	6	6	10	2	5	6	11	7	14	6	7	3	7	9
F	73	82	83	83	85	83	86	85	79	89	84	80	75	78	73	82	72	75	73	77
P	5	0	3	5	3	5	3	5	3	5	6	9	7	8	6	6	10	10	9	7
D	7	6	5	4	4	3	4	1	6	1	5	3	2	3	5	3	7	4	7	7
I	1	1	0	3	2	0	0	0	2	0	0	0	2	2	2	0	3	5	2	0

"A horizontal line appeared followed immediately by an oblique. There was vague movement over the field between the lines. Something moved from the first to the second positions" (Z, 8).

"Both lines appeared, distinctly separated and sharply defined. There was a pronounced impression of movement over the whole sector although each line was clear. There was no sensory blur" (C, 6).

"Distinct impression of two lines given practically together in time. There was movement from the upper to the lower position, but I saw both lines clearly" (K, 2).

In those cases of FM (full-movement) in which an object was apprehended as actually moving across the field, *O* often reported a line in various intermediate positions. The successive apprehension of these objects appeared to condition the perception of actual movement, or rather it meant that a line was seen moving across the field and occupying these successive positions. All *O*s reported these intermediate lines at one time or another. Occasionally 1 only would be reported; again 4 or 5. These lines were frequently observed to be quite like the initial and final lines. In fact, some of the reports indicate that the *O*s did not realize that only 2 lines were presented; instead they saw "several similar lines or the same line in several positions."

"A diagonal line appeared and moved to a lower position. Movement was smooth and uniform in rate except at a middle point in the descent where the line paused for a moment. At this point, I saw the line stand out. The remainder of the field was qualitatively like the background" (S, 8).

"The movement of a line across the field in three stages. The stages were clear. It was as if a line of light were emitted in sequence, or rather, as if a slit were being moved down and light came through the slit at these points" (B, 8).

"Movement very much as before. The lines were of the same length or perhaps it is the same line exposed clearly in four different positions. Movement was fairly smooth and uniform" (B, 10).

"I saw definite movement downward of a line; *i.e.*, I saw a line in different positions as it moved from an upper position to a lower position. The four intermediate positions were not so clear as the end positions" (K, 7).

"An oblique line appeared and moved to a horizontal position. There was a series of three intermediate positions between the upper and lower positions. Very rapid and continuous movement" (R, 5).

"I saw a line move from an angular to a horizontal position. There was first a bright yellow line against the black background. After it had persisted for a time it was cut off. After it was cut off it moved down smoothly to a horizontal position. I saw the line in various positions. I saw it there, and there, and there, as it moved. I saw nothing more" (Y, 9).

"I saw an angular line which moved from an upper to a lower position. I actually saw the line assume against the black background two or possibly three intermediate positions before it reached the horizontal position" (Y, 5).

In other cases, FM of a line across the field without intermediate positions was apprehended. Sometimes the bare apprehension of two lines appeared to carry the movement meaning. Quite often the field between the lines was reported to be illuminated in such a way as to aid in conditioning the movement perception; *i.e.*, as if a line moved across so rapidly as to leave a blur or trial of light.

"Both lines well defined but closer together, spatially and temporally, than before. Obvious movement of the top line as it moves quickly into the bottom position. Very little, if any, qualitative change in the field between the two positions" (C, 8).

"An oblique line emerged and moved into a lower position. As it moved it changed gradually from an oblique to a horizontal direction. Nothing else was to be observed" (M, 6).

"An oblique line appeared followed by a lower horizontal. Both clear. The upper moved rapidly to the lower. The field was unlighted between the lines" (Hs, 5).

"A line appeared in an angular position. It appeared to drift slowly to a horizontal position. The horizontal was broken in the middle. A faint haze which spread out from the lines appeared to cover part of the field between" (Y, 10).

Another outstanding fact as shown by Table I is the lack of any dependence of FM upon any particular temporal interval. Almost as many cases of FM occur at 225 σ as at 75 σ, the interval midway between the determined 'optimal' times of Wertheimer and Dimmick. At an interval where phenomenal simultaneity without movement occurred for Wertheimer, we found very insistent movement. At 225 σ where, prior to the development of our method, we had assumed that discrete succession would occur, 76 percent of all reports gave FM and less than 9 percent no-movement. Our results for this series indicate that it would be unwise to attempt to state that full-movement is to be obtained most often with any certain temporal separation of the lights. Slight variations in self-instruction might result in a complete shift of a small percentage of FM reports in favor of one interval or another. In fact our *O*s might see FM with an interval which 10 min. later gave 'no-movement.' There may be, it is true, slight qualitative differences in the character of the movement apprehended under rather widely separated intervals due to slight differences in the organic resources called into operation; but the predominant meaning with all of our intervals appears to be that movement occurs over the field between the two positions.

There appeared, moreover, to be no one interval or group of intervals which tended especially to condition either dual- or partial-movement. Dual-movement was reported in some cases under complete phenomenal simultaneity. Both lines appeared in the form of an angle which in turn moved up or down. At times, however, the two lines seemed to move toward each other,

"An oblique line appeared followed by a horizontal. Together they formed an acute angle filled with a dim yellowish light. The whole sector seemed to shift as a unit rapidly downward" (G, 7).

"I saw an acute angle. Both members of which were moving; *i.e.*, the top moved downward and the lower moved up as if to meet it" (Hn, 2).

"Two lines, an oblique and a horizontal shot out to the periphery and then moved slightly toward each other. A halo of light surrounded each" (Z, 1).

"Two lines given together. The spatial interval was dark. Both lines seemed to disappear almost simultaneously. The experience meant that an angle shifted up a few degrees and back again" (Y, 1).

"Both lines practically together in time. The field clear between. The oblique less intense than the horizontal. Both lines moved slightly up and then down" (K, 3).

On the other hand, dual-movement was sometimes reported where the interval was quite long. Here the movement assumed

various forms of which the most common was a completion movement of the second line as it moved away from the position of the first and into its final position. Dual-movement easily became partial-movement, depending upon the interpretation placed upon the last stage of the movement. At times *O* saw a line move down and disappear and then suddenly reappear lower down and move to a final position. It meant that the same line was moving ahead. In such cases, we can scarcely speak of dual-movement as it was originally used by Wertheimer. Rather it is a 'continuation' or a 'partial'-movement of one line.

"An oblique line appeared. It was moving rapidly downward when I saw it. After covering about ¼ in. of the field it disappeared into the blackness of the background. Then immediately another line appeared where the first had disappeared and went on down to a resting position. There was nothing in the field other than the lines unless it was a faint glow surrounding each line" (S, 10).

"A horizontal line appeared and immediately moved up about 5 degrees, and disappeared. Immediately I saw an oblique come in above where the horizontal disappeared and move on up to the final position. Field between the end positions was dark except for the intermediate lines" (R, 10).

Partial-movement of one object, the other remaining at rest, was frequently perceived. It appeared indifferently in connection with either, though possibly the second may have been more often observed to move.

"An oblique line appeared but stationary. It started to move downward just as I saw it. Then a horizontal line appeared below. The upper line moved only part (¾) of the distance between the lines. I saw nothing more" (Y, 5).

"The oblique appeared but remained at rest. Immediately following its disappearance, a line nearer the horizontal than the oblique position came in and moved rapidly down to the position of the horizontal" (Y, 10).

"An oblique line appeared followed by horizontal. The horizontal persisted a trifle longer. Distinct movement referred to the horizontal line as it moved into its place over about ½ of the intermediate field" (G, 9).

"Two lines came successively. There is movement here but only in the upper part of the sector as if the oblique appeared after the horizontal and about ⅓of the way up (I saw a line here) and moved on up to the final position. The oblique seemed, then, to swing up over ⅔ of the sector. There was nothing visual between the lines" (K, 8).

Movement within a given figure was seldom observed. Most of the reports were given by Z. The movement was usually experienced as a flowing, a shooting, or a twisting within the line.

"Two lines appeared in succession; a horizontal and then an oblique. The first was dim; the second was clear. There was a flowing movement within the second line,—perhaps back and forth. I am unable to state direction with certainty" (Z, 10).

The effect of repetition upon the apprehension of movement is to be seen (Table I) in connection with the reports of M and Z. By the time Series IV was reached each *O* had had considerable practice. In Series IV (A) only 2 reports of 'no-movement' out of a possible 40 were given by these *O*s.

This fact is obscured by the combination of Series II and IV; but the total of these two shows less 'no-movement' than Series I and III. Practise appears, then, as the factor to which, with the greatest degree of assurance, we may attribute to these two *O*s this difference in visual apprehension at earlier and later periods. It has frequently been mentioned that it requires time and training in order that some individuals may apprehend stroboscopic movement. This was not true of our other *O*s. As may be seen, most of them immediately experienced movement. This is not to be ascribed, either, to a neural set or predisposition created by a particular experimental atmosphere. Some of our *O*s knew nothing of the character of the problem. The same holds true for those persons who happened to visit the laboratory. Out of a score or more who observed (without formal instruction) not one failed to report movement.

No significant difference appears where the direction of movement is upward. There are, it is true, fewer cases of FM; but there are, likewise, fewer cases of 'no-movement'.

It would be interesting to compare a large number of cases of movement in these two directions with various distances intermediate to the two lines to determine whether there exists any real difference in the fullness of movement or of the apprehended distance between members.

If such a difference in movement should exist it would be quite significant for those theories of visual movement which stress eye-movement. Since the eyes move downward with greater ease and rapidity, one might expect a larger percentage of FM reports in connection with the a-b presentation than with the b-a sequence.

Series I-IV; with Object B. The quantitative results for this series are given in Table II. There is a very close similarity between this set and Table I. The experiments with B (parallel horizontals) tend merely to substantiate the facts ascertained in the 4 series with A. FM was apprehended in a large percentage of cases at all intervals and by all *O*s. The one arrangement gives as clear FM as the other.

We find our results in this connection in close harmony with those of Wertheimer and Dimmick. They, too, discovered no significant difference between the two arrangements. There is, of course, nothing to lead one to assume that any real difference in visual movement should accrue to the use of either stimulus-pattern so long as the apprehended distance between successive members of B is not excessive.

Thus far we have not dealt specifically with the qualitative aspect of the movement-experience. Throughout the entire experiment the predominant appeal was to the visual resources of the organism. Owing to the method of stimulation very little qualitative variety of a visual character was to be expected. Quite often merely two yellowish streaks, with or without a halo of dim light, were observed. Various *O*s reported from time to time that the second line appeared a little brighter; as if the first had been added to it, increasing its intensity, or as if the line had suddenly stopped, allowing its spatial character to be better seen. At times, the field between the lines was distinctly different in quality from the background. It seemed partially or wholly filled with a 'hazy,' 'cloudy,' 'dusty,' 'filmy' light. This light might be uniform in distribution; again it might

be very patchy, even streaky with decided intensive differences. The light was often unstable and shifty, due to pronounced changes in intensity. It would shoot, dart, stretch out or contract in such a way that it served directly to condition the perception of movement. At other times it was just *there*, resembling Dimmick's visual grey. In so far as it had to do with movement in these latter cases, it appeared as if it were the result of retinal displacement, the blur that comes from a rapidly moving object or the cloud raised by some actual movement. It was, then, one of the visual resources directly aroused on occasion by the stimulus-objects and apparently utilized at times by the organism in apprehending or in qualifying visual movement. We have tried in the general discussion of our results to indicate the exact importance for the apprehension of movement of this visual filling.

Supplementary to the visual qualities, and playing, as we believe, no insignificant part in the perception of movement may be mentioned (*1*) verbal kinaesthesis, (*2*) fairly specific pressures around the eyes and face, and (*3*) a certain rhythmic activity of bodily members, particularly of the feet and hands. Some *O*s report that the expositions were subvocally accented, as 'ta-*ta*', 'there-*there*.' Others find tensions in the feet, as if the heel were put down, followed by a sharp slap of the rest of the foot. Non-verbal lingual movements not unlike a 'pursuit' appear at times with the perception of movement. Whether these various kinaesthetic factors are important for the experience is difficult to determine with certainty. Reports of them seemed chiefly to occur when only the bare lines were apprehended.

A report from Professor Bentley runs as follows "Successive lines. General apprehension of downward movement. Whether it is a meaning built up out of strains I get around shoulders and arms or whether I translate the strains into movement, I don't know" (8).

"Distinct verbal judgment 'It moves. Oblique moves into its place.' There was no phenomenal visual basis" (G, 10). "Verbal judgment 'the upper line jumped into its position' " (G, 7). "Movement was in terms of eye-movements (*i.e.*, nothing visual)" (G, 4).

"I accent the 2nd member. Movement comes, I believe, kinaesthetically. Movement is quite insistent without visual basis" (C, 8). "Very good movement. Kinaesthetic factors were largely responsible for this" (C, 7). "Visually there was no basis. Good movement, however" (C, 8). "Definite movement over the whole angle but not given visually" (C, 6). "I tend to follow the whole performance with my tongue. My tongue moving helps to give me clear visual movement" (C, 10). "Kinaesthetic factors persist right along" (C, 4). "There is a decided tendency to vocalize, to sing two tones of different pitch 'high-low,' 'high-low' " (C, 7). "Distinct eye-movements. I follow the line. Movement comes in other terms than visual in which I get very distinct impression of two lines, but first moves to second" (C, 10).

"Very strong kinaesthesis around eyes and head which meant 'there, there;' with movement from one line to the other" (G, 10). "The experience is accompanied by very clear verbal kinaesthesis and diffuse pressures about the head and chest meaning 'there, there.' These factors,

I believe, are largely responsible for the perception of movement" (G, 7). "Definite movement from below upward; little or no visual basis; given, no doubt, kinaesthetically" (G, 4; G, 3). "Rhythmic appearance of three horizontal lines "ta-ta-*ta*' " (G, 3). "I observe a great deal of kineasthesis around eyes and neck. There is constant verbal kinaesthesis meaning 'there-there;' 'moving up;' 'going down' " (G, 1).

"At times, I am sure that as much as I try I am unable to keep my eyes on one spot. I feel my eyes jump when the line comes on" (Z, 2). "Movement over whole sector. Given kinaesthetically" (C, 5). "Two lines in definite sequence and each with a slight halo. Definite movement from one to the other carried largely in non-visual terms" (G, 10). "A number of times I have the feeling of putting my heel down followed by a sharp slap of the sole" (C, 4).

Once more, the visual movement appeared to differ in a qualitative way under different intervals. With short intervals the movement was 'vigorous,' 'lively' and 'insistent.' It seemed to be smoother and more uniform in rate; there were fewer reports of secondary positions. The movement was carried more fully, as it appeared, by visual processes. It was more pronounced, but, we may say, less the movement of a 'real line' than with the longer intervals.

Nevertheless, there was more of a 'turning movement around a pivot' (with object A) reported with the shorter than with the longer intervals, where the lines usually failed to meet at an apex. The movement of the longer intervals was irregular and jerky. There were more stages to it; that is to say, there were more intermediate lines or positions than in the shorter intervals. Such lines generally meant a slight pause in the course of the movement. Where the interval was short with few, if any, intermediate lines, there was usually a leaping movement, a short, quick jump, or a rapid sweep from one to the other position. Where the intervals were long, and intermediate lines absent, the *O*s reported a drifting, floating movement or a slow, easy, flowing movement across the field. This type was never reported in connection with the shorter times. As we have just said, in connection with the longer but seldom with the shorter intervals, all *O*s reported now and then that the two lines (arrangement A) did not meet, or that the oblique had been changed to a vertical. At times, the oblique even bisected the horizontal line.

From our observations in these 8 Series we feel safe in asserting that full visual movement appeared to be conditioned by the successive apprehension of two lines in different positions with or without a qualitative intermediate filling, or by the subjective re-enforcement of the intensive aspect of the second member with the meaning that the first was added to it, or that the second had now come to rest and thus could be clearly seen. Along with these visual resources must be mentioned general kinaesthesis, which constitutes at times, as we believe, an important if not a dominant resource of the organism in the apprehension of movement under our conditions.

What is to be said of dual and partial-movements? Under the same physical conditions that gave FM, *O*s occasionally reported (a) full-movement of one member with slight movements of the other, (b) partial-movements of both, or (c) of one

member. Dual-movement, as we have said, appeared under simultaneity, complete succession, or full-movement.

It was difficult to find any specific factor to which it could be attributed. The phenomenal basis was very elusive. When both lines were clearly apprehended, with the intervening field uniformly lighted, there might be distinct movements of each line. This same thing happened when the lines were much the same with the field dark between them. Some cases of dual-movement were reported with the meaning that the upper line moved down knocking the lower line out of its position. Here we have a combination of full- and dual-movements. The dual-movement in these cases seemed to be chiefly conditioned through the apprehension of two lines close together at the bottom, much as would occur if one object thrust another aside and assumed its position.

A form of dual-movement or a modification of full-movement occasionally occurred with the meaning that, because of rapidity of movement, the moving line overshot the position of the second member and was forced to move back to get into its final place.

"Upper line went down beyond its final resting position only to withdraw to it" (Y, 6). "I can't distinguish whether the top line moved part way past the bottom and then back to it or if the lower moved, or if both went down and back after the first is joined to the second" (C, 4). "Movement of lower line beyond the upper line and back again to it" (G, 1). I saw the line in 5 positions in its downward course. It did not stop with the bottom line but went past it and then came back" (B, 2). "Upper line moved past the bottom and back to it" (C, 1, 2, 4, 6, 10).

Partial-movement differed little from FM, except in extent. It was usually described as a swinging, a darting, a jumping, or a jerking movement of a line. At other times it was a smooth, slow, flowing movement over a part of the field. At still other times, it was just movement, often vague and unlocalized, around some part of the total figures. Very seldom was there a report of a 'trembling or shaking' of one line. This sort of movement generally occurred in dual form.

Concluding our discussion of the first 4 Series with the angle and the horizontals, we shall have to consider for a moment the apprehension of FM under conditions in which phenomenal simultaneity seemed to predominate. According to Tables I and II, full movement was reported in a large number of cases with intervals of 15, 30, and 45 σ. But the Tables fail to show that, as a rule, the figures were *seen together* in the form of an angle or parallel lines, but with movement; and often the movement of a line, although the line might not always be seen in intermediate positions. It is to be noted that some *O*s reported most insistent and pronounced movement where there was simultaneity or approximate simultaneity. In the light of former investigations upon the visual apprehension of movement this fact is of peculiar importance[23]. Its significance will

[23]Professor Koffka, who has done much fruitful work in this field of perception, visited the laboratory during the course of the experiments. Upon observing for us, he was immediately impressed by this form of movement. He suggested that since it had never before been observed, it should be distinguished in some way from the other types of movement previously described by Wertheimer and by his own pupils.

be considered when we come to our general discussion. At present, we wish merely to call attention to its general character as given in a few of our own reports.

"Very strange, indeed. I saw an angle almost whole, but the upper line moved down to the lower." (Professor Koffka).

"Two lines together. The upper disappeared followed immediately by the lower. There was a weak yellowish light between the lines. I saw very definite movement downward of the oblique as it swung to the horizontal position" (Y, 1).

"A photographic representation of an acute angle with just a smudge of light yellowish light of low intensity at the innermost corner of the angle. The diagonal swept rapidly and smoothly to the horizontal" (S, 1).

"Two lines clearly given forming an angle. The field between dark. Very fast and continuous movement from upper to lower position. The whole experience is highly unified" (C, 3).

"Two lines at an angle. The lower line dim and narrow. The upper clear and wider. Field between clear. There was definite movement from below upward over the whole field of the horizontal line" (Z, 1).

"An angular line with a horizontal below it. The field was a little hazy around and between the lines. I saw very rapid movement of a line from one position to the other" (Y, 2).

"Two lines together forming an angle of about 45°. Very nice movement (just as if a fan opened up). I saw a line swing up over the field. The field between was qualitatively unlike the background. A weak light surrounded the lines and filled the angle" (K, 1).

Throughout Series I-IV the whole period, including the exposition times and the interval, was extremely brief. In some cases the entire presentation occupied little more that 1/10 second (115 σ). The longest time was only 325 σ. Under these conditions accurate apprehension was so difficult that the reports were often made with considerable difficulty. Even with the shortest times, however, two lines or rather two positions were always apprehended. Along with these lines the intermediate field might be partially or wholly filled with a 'hazy,' or 'filmy' light. But the field might also be dark, with just as insistent an impression of movement as where it was qualitatively different from the background. With the very short intervals the perception of movement appeared to come almost before the lines were distinctly apprehended, or else the lines were given with movement just at disappearance. Aside from the contribution to the perceptual performance of kinaesthetic factors, we believe that the slight sequence that must necessarily have occurred in the presentation of the figures accounts for the apparent perception of movement before the lines were clearly apprehended. Thus *Os* sometimes spoke of movement after which the whole figure stood out clearly; or movement occurred and was followed by a faint 'after-image' of two stationary lines. The movement that came simultaneously with the disappearance of the figures was due partly, as we surmise, to the fact that the first, being presented a little earlier than the second, disappeared a few sigma before the second; but owing to the extreme rapidity of the whole presentation these slight differences were unobserved. The effect upon visual apprehension, however, was important. It meant that the first line did not simply disappear, but that it moved down to the position of the second member, going out by way of this member. Assuming this to be true, we sought a more satisfactory method of testing this whole matter of the apprehension of movement under conditions in which the disappearance of one object was by way of the remaining, or in which the appearance of a second object was by way of the first. To this end we proceeded with the method and the conditions of Series V and VI (already described) where observations could be made with a greater degree of assurance and accuracy.

Series V; addition of one member to another (Objects A and B). In this phase of the experiment we sought to determine the nature of visual movement in connection with the addition of one figure to another. No preliminary series were given. The *O*s were unacquainted with the setting. There was no interval. *O* was presented with one figure and then the other was instantaneously added to it while the first remained. The results are given in Table III.

TABLE III
Series V; Addition

	Object A						*Object B*					
Order	*a*——*b*			*b*——*a*			*a*——*b*			*b*——*a*		
O	O	F	P	O	F	P	O	F	P	O	F	P
G	1	4		1	4			5			5	
C		5			5			5			5	
Y		3	2		5			2	3		1	4
Z		5		2	3			5			4	1
R		5		1	4			5			5	
S		5			5			5			5	
Hn		4	1		5			5			5	
K		5			5			5			1	4
H		5			5			5			5	
M		1	4		5			5			1	4
	1	42	7	4	46			47	3		37	13

Here we find but 5 cases of 'no-movement' reported out of 200 observations. Most of the *O*s apprehended FM in all cases. There were no reports of dual-movement or of movement within a member, and only a small number of partial-movements occurred. The conditions in this and the following series were almost ideally suited for the perception of visual movement. Several *O*s commented on the ease with which observation could be made in this as contrasted with the former series, and on the insistence and clarity of the movement. The outcome was not wholly unexpected; yet there was an element of surprise. There is little in the literature on visual movement to lead one to assume that the mere addition of one member to another, to give a particular object-configuration, would be accompanied by experiences such as described in the following reports.

"I saw an oblique line on dark background. Then suddenly a bright line separated from the oblique and 'fell' down to a horizontal position forming an angle. The angle persisted for a moment and disappeared. The field was clear between the lines" (Z, 1).

"There was a horizontal line on a black field. Then movement downward. Then another lower horizontal line. Then upper line reappeared. It was not there during movement. Then both lines disappeared. Nothing visual here in the field between the lines to give me this movement" (Y, 3).

"A horizontal line appeared. Then a line suddenly detached itself from this line and moved up to an oblique position. Then I saw an angle which soon disappeared. There was nothing visual other than this" (S, 1).

"An oblique line appeared. Then it dropped rapidly and smoothly to a horizontal position. Then I saw an acute angle which disappeared. There was nothing more visually" (K, 1).

"I saw an oblique line on dark ground. It then moved all the way down to a horizontal position, but strangely enough when I saw the line

in the horizontal position I again saw the oblique forming an angle with the horizontal. (It struck me as odd that the oblique could get down and then back again so quickly and without my seeing it go back)" (R, 1).

We have here pronounced FM without any sequence of objects in the physical sense of one line appearing and disappearing, to be followed by another similar object, which in turn disappears. Yet under these new conditions we obtain approximately the same kind of movement as we secured under the method of successive presentations. We cannot say 'exactly' the same kind because movement was clearer under these conditions, with particular emphasis upon FM. This movement of a line from the position of the first to form another line is exactly what we obtain under the usual method of stimulation where one line is followed by another which appears to come from the region of the first and to move to its final position. This series sheds considerable light upon the movement that is reported as coming before the objects are clearly seen and also of the movement that has been so often reported in some such way as the following; "A line appeared and then another moved from it to form a second line."

Certain reports in connection with Series V are very interesting from a theoretical point of view. I give the following reports from Y.

"A reddish-yellow line appeared on a black ground. Then a bluish-green line appeared just above it when it disappeared and moved quickly to an upper position. Then two yellowish lines stood out for a second and then disappeared." "A yellowish horizontal line appeared on a black field. Then a low chroma bluish-green line started just above it and went on up to its final position. Both lines then disappeared."

The implication appears clear to us. It means that here an instantaneous shift upward occurred in the point of fixation at the time the figure was added and during this change the negative after-image of the first line appeared. This change in fixation also might serve to account for the experience of visual movement. As we know, there would be sufficient time for this shift to occur, as Mach proved with his work under instantaneous exposition of adjacent objects.[24]

Series VI; subtraction (*Objects A and B*). Here one member was subtracted from the total configuration A or B. The quantitative results are summarized in Table IV. An inspection of them shows the very close similarity to those of the preceding Series (Table III). Under the new conditions the *O*s did not, curiously enough, see a line disappear into the blackness of the

[24]Mach exposed instantaneously two small squares of colored paper (2 x 2 cm.), 8 cm. apart with the instruction to attend to the right. The *O*s reported that the square directly attended to was red, but that the other was greenish. It was pointed out that a shift in attention from the one to the other might account for this difference in hue, although the whole period of exposure was very short (that of the electric spark). Mach, E. *Beiträge zur Analyse der Empfindungen*, 1906, 205-6.

background, or merely fade out; they saw it go out by way of the remaining member. It is either a case of no-movement, full-movement or partial-movement.

"Both lines given in the form of an angle. The oblique seemed to fall into the horizontal. There is some change in the field but I am unable to describe it" (G, 3).

"Two parallel lines on black field. Then the upper line suddenly moved downward to the lower. Nothing given visually to give me this movement so far as I am able to observe. The line just moved" (S, 1).

"I saw two horizontal lines with a gauzy light between them. The lower then moved up to the top line and was gone. It seemed to 'swoop' up the film of light as it moved up" (C, 1).

"I saw an angle and then the oblique 'clapped' down to the horizontal. Very good clear movement. The upper line just moved down and closed up. Nothing between the lines filling the field" (Z, 1).

"Two horizontal lines with a faint grayish haze between them. Then there was a peculiar dropping of the upper horizontal line to the lower in a quick smooth manner" (K, 1).

TABLE IV

Series VI; Subtraction

	Object A						*Object B*					
Order	ab——a			ab——b			ab——a			ab——b		
O	O	F	P	O	F	P	O	F	P	O	F	P
G		3	2	1	4		3	2		2	3	
Z		5			5			5			5	
Y		2	3		5			5			5	
C		5			5			5			5	
S		5			5			5			5	
K		1	4		5			3	2			5
Hn		3	2		5			5			5	
M			5			5		5			5	
R		5		2	3			5			1	4
H		5			5			5			5	
		34	16	3	42	5	3	45	2	2	39	9

Summary of Series V and VI. So far as we are aware visual movement under the precise conditions outlined in Series V and VI has never been reported or described. Under all previous conditions objects have been presented in sequence with well-known results. Here the conditions were different. *O* had an opportunity to attend fully to one member of an object while the other was being added, or to a whole figure while one member was removed. There was no temporal interval between the addition or the subtraction of members. The whole procedure was relatively slow, except for the sudden appearance or disappearance of the member.

Possibly this was the reason why the *O*s expressed greater satisfaction in observing. There were no reports of uncertainty and no need of repetition. In Series I-IV the *O*s often complained of the extreme rapidity of exposition and the general feeling of uncertainty concerning the accuracy of report. In these series there was neither objection nor uncertainty.

The apprehension of movement under the above conditions is significant. The change in the hue of the lines, together with

reports upon the momentary disappearance of the first object during the apprehension of movement, throws further light upon the functional resources of the organism utilized in the perception of movement. We hesitate to advance a theory or to develop explanations. We recall the reluctance of Exner, Wertheimer, Koffka and others to explain visual movement by any reference to ocular shifts. Yet it does appear from our work with the addition and subtraction of members that very clear and insistent visual movement might arise from rapid eye-movements.[25] This would account for the fact that one line disappeared for a short time when movement occurred. It would also explain, as we said, the qualitative change in the line of light. Furthermore, it would account for the failure of the oblique and the horizontal to meet at the proper point. Finally, it would explain the apprehension of the line as moving beyond, and returning to, its true or final position. If this interpretation is correct, it means a great deal to current theories of visual movement. Our conditions in these two Series (V and VI) are in effect similar to these involved in the usual method of successive presentations, but they permit us to ascertain with greater clarity the character of the visual movements.

Series VII; o σ interval (Objects A and B). Earlier in this paper was pointed out Hillebrand's claim that movement can be obtained where the interval is less than Wertheimer believed necessary for the apprehension of movement. In the present series we sought to determine the character of perceived movement (if it existed) with o σ interval and successive members.[26] The quantitative results are given in Table V. Here for the first time we fail to find any report of 'no-movement.'

TABLE V
Series VII; o σ Interval

Order	*Object A* a—b		*Object A* b—a		*Object B* a—b		*Object B* b—a	
O	F	P	F	P	F	P	F	P
G	4	1	3	2	5		5	
K	5		5		5		5	
M	5		4	1	5		5	
Y	5		5		5		5	
Hn	5		5		5		4	1
C	3	2	1	4	5		3	2
S	5		5		5		5	
Z	5		5		5		5	
R	5		5		4	1	5	
H	5		5		5		5	
	47	3	43	7	49	1	47	3

[25]R. Dodge, Participation of Eye-Movement in the Visual Perception of Motion. *Psych. Rev.*, 11, 1904, 1-14.

[26]Here we used the same sort of cards and drawn figures as Dimmick employed.

Only full- and partial-movements were observed. The first sort predominates; 9/10 of all reports indicate FM. Moreover, the experience was just as insistent and as clearly apprehended as under the intervals employed in Series I-IV.

Hillebrand is the only person, unless we may except Benussi, who has heretofore reported full-movement under o σ interval. Wertheimer, as we know, found that simultaneity without movement occurred as the interval approached o σ. Intervals of about 30 σ could be counted upon to give resting figures or, at least, with slight unrest only in each member. Partly to account for this alleged fact he desired his cortical theory. We do not wish now to discuss the significance of our results in the light of the current theories; that is reserved for our general discussion. We wish merely to present at this time a few reports with o σ interval to be considered in connection with what we have just said about the significance for Wertheimer of the temporal interval.

"I saw a yellowish field with a dark grey oblique line on it. The field suddenly became much brighter and there was a horizontal line in the field instead of the oblique. I saw a single line move rapidly from an upper to a lower position. There was nothing between the two positions visually to give me this movement" (Y).

"An upper diagonal line moved down, or rather swept down to a lower position. The field at the close was brighter than at the beginning" (C).

"A horizontal line appeared. Then, after a moment, there was rapid upward movement referred to the region above it and to the horizontal line. The line stopped in an oblique position and then disappeared. At the moment that movement occurred the field grew much brighter. There was nothing between the lines of a visual character" (G).

"The only thing I can say is that the oblique 'clapped' down to the horizontal" (Z).

"A horizontal line appeared. Suddenly it moved up to a diagonal position and then disappeared. Along with the movement the whole field was much brighter. There was nothing visual to give me this movement across the field, *i.e.*, there was no particular visual filling between the lines. The whole rectangular area was lighted" (S).

"A black horizontal line on a dim yellowish field. Then a sudden increase in the intensity and I saw a horizontal line above the former position of the first. Just at the moment that the field brightened, I saw the lower line rise smoothly up to its final position (2 cm.) above. There was nothing in the visual field between the lines to give this" (Y).

Series VIII; continued exposition (Object A). It has been claimed by various investigators that the best visual movement is often obtainable under recurrent stimulation with the same object-pattern. Individuals who were unable to perceive movement under single expositions readily saw full-movement under repeated exposures. To ascertain the nature of movement under these conditions we conducted the present series.

For the o σ interval we used the same method as in Series VII, except that the weaker light was not removed.[27]

After the proper signals the control key was pressed and released at 1 sec. intervals, giving in this fashion 5 expositions of each figure. For the remaining 10 intervals the slitted cards were placed at P_1 and P_2, the disc set to give the desired interval, the motor started and the control key pressed while the disc rotated 5 times.

The results are summarized in Table VI. Here, as in the previous series, we find FM predominating. In fact, in no case was there an absence of it. It is quite evident that under other-

TABLE VI
Series VIII; with Object A

O	0	1	2	3	4	5	6	7	8	9	10
	0	15	30	45	60	75	90	120	150	180	225
	F D	F D	F D	F D	F D	F D	F D	F D	F D	F D	F D
S	3–	3–	3–	3–	2– 1	2 1	2– 1	3–	3	3	3
Z	3	3–	3–	3–	3–	3–	3–	2– 1	2 1–	3	3
Hn	3	3–	3–	3–	3–	3–	3–	3	3	3	3
C	3	3–3	3–	3–	3–	3–2	3–	3–	3	3	3
R	3	3–3	3–3	3–	3–	3–	3–	3	3	3	3
Y	3	3–	3–	3–	3–	3–	3–	3–	3–	3–	3–
G	3	3–	3–	3–	3–	3–	3–	3	3	3	3
H	3	3–	3–	3–	3–	3–	2– 1	3–	3	3	3
K	3	3–	3–2	3–	3–	2– 1	3	3	3	3	3
M	3	3–3	3–3	3–	3–	3–	3	3	3	3	3

– represents movement in one direction. All others indicate movement in both directions.

wise similar conditions the effect of repeated exposures of a stimulus-pattern tends to make for very clear perception of movement. In the preceding Series, the *O*s reported two lines. One appeared and moved up or down to a position where it could be seen as a resting line or a line in a given position, or both lines were seen with movement between them or in connection with one. But in the present series, the meaning was, as a rule, slightly different. More often it meant that a single line moved up and down, or a line recurrently appeared and

[27]This work with a stronger and weaker light had an additional value. It might have been objected that the increase in the intensity of the second light (when we were working with either single or repeated exposures) might serve to facilitate the course of the *phi-function*. Here we found that the movement given by the disappearance of the strong light with the instantaneous appearance of the weak figure was just as clear and as insistent as when the strong light wiped out the weak.

moved over and out of the field. The two lines, as end-positions, were less clearly perceived. The whole experience appeared to be highly integrated, approximating, at times, the movement of a physical body.

With these continuous repetitions we had an opportunity to compare visual movement with relatively long and short intervals. Always there were two intervals, a long (C) and a short (D).

When the radial sectors were close together C was quite long, as much as 885 σ; When the sectors were most widely separated C was about 675 σ. While we did not deviate from our regularly used intervals to test others, this method seems to us the best way of investigating the relative importance of various intervals for visual movement.

In any other method, the time between presentations due to the necessary shift in apparatus is too long for reliable comparisons. Aside from the introduction of the effect due to repetition this is, as we find, the safest and the easiest way of securing a large number of reliable judgments for a comparative study of movement, especially where the primary object is the determination of the interval best suited for movement. This form of stimulation also furnished us with a more satisfactory method of ascertaining the nature of the phenomenal bases of movement. *O*s were able to see the same thing occur 5 times in rapid sequence. In this way they could verify certain phases unclearly perceived under previous expositions.

We have seen that, in the preceding Series, the *O*s apprehended FM (*1*) where only two qualitatively similar lines were present; (*2*) where the two lines were present with the second intensified; (*3*) where the intermediate field was partially or wholly filled with a subdued light; and finally, (*4*) where a varying number of intermediate lines or positions were observed. This phase of the experiment gave us a chance to verify these reports and to note further visual and non-visual foundations for movement. It is understood that we recognize the danger inherent in any attempt to compare two sets of results obtained under dissimilar conditions. Since, however, in this series we kept everything present in the proceeding and added merely the recurrence of the same pattern, the results which we obtained may safely be used toward a further understanding of our earlier experiments. We present for inspection reports taken from this series.

"Very good movement up and down of a single line. The upward movement was quick and light. There was nothing in the visual field. In the downward movement there was the step-wise effect. The movement seems more deliberate and more ponderous, because of the number of intermediate positions. The movement was not wholly smooth" (C, 8).

"Very good downward movement of a line with less insistent upward movement. I do apprehend the line going upward; although I am unable to see anything in the field. Upward movement is much faster than downward. The line see seems literally to 'snap' up and then 'flutter', *i.e.*, there are distinct positions assumed by the line during downward movement but nothing in upward movement" (K, 7).

"A line was seen to move downward 5 times over an angular space of about 45°. This happened 5 times. There was no upward movement. During part of the time that I saw movement there was a yellowish haze over a small part of the area between the upper and lower limits of movement; the rest of the time there was nothing" (Y, 6).

"A line moved fairly slowly up and down over an angle of about 45°. The whole angle was dimly lighted. The downward movement is clearer than upward; it was almost as if I did not see the upward movement; yet I am positive that I did" (S, 9).

"A diagonal line moved rapidly up and down. There was a dark figure between the upper and lower limits. This figure was wide at the top and bottom and narrow in the middle. It was darker than the surrounding area" (S, 5).

"A single line moved from the horizontal to the oblique position and back again. A very clear 'clapping' down and up. All clear and insistent. Nothing in the field between the upper and lower positions so far as I could see" (Z, 10).

"Very rapid and obvious movement up and down of a single line over an angular distance of about 45°. I did not see anything in the area intermediate to the upper and lower limits" (C, 0).

Here we find a difference in the character of movement emphasized where there is some slight observable difference in the phenomenal basis. Light is thrown upon the movement where first one line and then another is apprehended. In all the cases in Series VIII, except those with $o\ \sigma$ interval, the time between the exposition of the second member of the preceding and the first of the following pattern was relatively great, yet upward movement was seen at some intervals by all *O*s except Y. In the majority of cases the movement upward (interval C) was unlike that downward (D). We pointed out earlier that where the interval was longer and nothing was seen in the intermediate field the movement assumed a 'quicker', a 'lighter', or a 'floating' character. We find this to be true of interval C in a great number of cases in the present Series. The line moves without pause or jerk from one region to another. In interval D frequently appeared intermediate positions, and the movement was more irregular, more jerky and usually slower. Pronounced movement of a smoother and more uniform nature was found where the interval was filled with a dim light which gave *O* the impression that something had passed over the field leaving a haze. It did not mean necessarily that a line was seen to move through this haze. The filling served merely to reënforce the impression of movement and to give a greater feeling of assurance. This seems to be the common effect upon the movement experiences of such visually filled space.

When there was little or nothing in the field intermediate to the lines, the movement could be just as clean-cut and as impressive as under the above described conditions. Only the

movement was quicker, smoother and lighter. Other resources than visual were—as it appears—involved. *O*s spoke of diffuse pressures around the eyes, tongue and throat, and also of subvocal speech. In speech there was the same tendency as before to accent one member: *ta-ta, there-there, up-down, down-up.* Moreover, we must not underrate the significance of rhythmic tendencies. *O*s reported tensions in the head, hands and feet, meaning incipient movements of these members. Further proof of this rhythm is given in those reports where movement was better in the last 2 or 3 of each group of 5 distinct exposures. Before the end of each group was reached, the *O* appeared to be aided in apprehending movement by falling into the rhythm of presentation. Another fact brought out sharply by Table VI furnished additional proof of the significance of rhythm. It is to be noted that the smaller D-intervals (aside from o interval, where the presentation was different) there was full-movement in one direction only. Every *O* took two expositions as a unitary whole separated from the next two by an interval of blackness. But when the difference between the C and D intervals was lessened, the reports were modified, indicating a linking-up of the 5 double expositions into a fairly unitary sequence with the meaning of movement up and down; and the verbal and somatic rhythms appeared to be more easily conditioned by sequence in which the intervals were more nearly equal in length, so that when the D-intervals were lengthened and the C-intervals proportionately shortened, the rhythm was more insistent. This factor of rhythm is indicated in connection with o interval. Here there was succession with equal intervals as well as equal exposures. One object was given for 1 sec. and then the other for a similar time. This was repeated 5 times. *O*s were seldom able to report anything in the intermediate field; but FM of an insistent character was reported in all cases. Z stated that this was the best of all observed movement. We find that the two sets of facts obtained from widely separated intervals (o σ to 225σ and upward) point to the same thing, the utilization by the organism of resources, in addition to the purely visual, to condition clear movement of a visual character.

It is not to be assumed that the relevance of these 'supplementary' factors in perception was always clear to *O*. It is fully recognized that many clearly observable kinaesthetic factors often occur, not as essential constituents in the perceptual incorporation, but as chance concomitants of perceptual performance. Since the *O*s so often experienced a decided lack of all clear-cut visual factors for the movement-perception, and since they repeatedly mention kinaesthetic factors, we seem justified, however, in accepting the possibility of coöperation of these somatic items in the apprehension of movement.

Series VIII; with Object B. There is no outstanding difference between our present results (Table VII) and those with A (Table VI). There were, to be sure, more cases of dual-movement, but most of the cases belong to 2 *O*s (G and M) and the number is not large enough to be significant. In all cases save one, o interval gave full-movement in both directions.

TABLE VII

Series VIII; with Object B

O	0	1	2	3	4	5	6	7	8	9	10
	0	15	30	45	60	75	90	120	150	180	225 σ
	F D	F D	F D	F D	F D	F D	F D	F D	F D	F D	F D
Y	2 1–	3–	3–	3–	3–	3–	3–	3–	3–	3–	3–
C	3	3–3	3–3	3–3	2 1–	2– 1	3–	3	3	3	3
S	3	3–	3–	3–	3–	3–	3–	1 2–	2 1–	2 1–	3
H	3	3–	3–	3–	3–	2 1–	2 1–	3–	3	3	3
K	3	3–	3	3	3	3	3	3	3	3	3
Hn	3	3–	3–	3–	3–	3–	3–	3	3	3	3
R	3	3–3	3–3	3–3	3–	3–	3	3	3	3	3
G	3	3–2	3–	3–	3–1	3–	1 2–	2–1 1	3–	3–	3
Z	3	3–	3–1	3–	3–	3–	3–	3	2 1–	3	2 1–
M	3	3–3	3–3	3–3	3	3	3	3	3	3	3

– represents movement in one direction. All others indicate movement in both directions.

"Rapid movement of a line (dark on white expanse) down and back again" (Y). "The black line moves up and down" (Z). "Very obvious movement up and down of a dark grey line" (C). "A dark horizontal line moves up and down" (S). "A greyish horizontal moves up and down rather rapidly" (K).

The smaller D-intervals, with 1 or 2 exceptions gave FM in one direction. Along with these reports there were a few on dual-movement. *O* stated that one line moved away as the other approached, or a line moved down and then part way back.

"Upper line moved down to the lower in two steps (two intermediate positions). Then lower seemed to move slightly upward, but am unable to state anything about the visual field when this took place" (C, 3). "The upper horizontal swings down over intervening space to lower and then lower moves down slightly" (C, 1).

"Two almost simultaneous streaks. Downward movement of the first to the second. Then the lower moved slightly down, too" (G, 4).

"Two parallel lines moving. The upper when it reached the bottom, again appears at the top and moves down, as it does, the second moves down a little" (M, 3).

The longer D-intervals gave FM either one way or both. On the whole there was nothing new by way of qualitative variety in these longer in-

tervals. The *Os* reported visual resources supplemented by rhythm, verbal kinaesthesis, ocular pressures and subvocal speech, all of which combine to give FM.

"A series of apparent units. Same thing over and over. A path of yellowish light filled a small part of the field between the upper and lower positions. I saw a line move downward several times. There was a definite rhythm in the movement referred to weak pressures around the eyes and head. Movement was smooth and recurrent" (Y, 10); "Movement was jerky and in units. I saw three definite intermediate positions of the line as it moved downward" (Y, 9); "Movement downward was irregular. A slight pause at each of three fairly bright intermediate lines" (Y, 8).

"A decided dipping down and going up of a single line. Movement down was not smooth. I saw several distinct phases of the same line. The movement back was just a quick jump without anything between" (Z, 9).

"A horizontal line which moved down and disappeared. This was repeated several times. There was nothing in the field between the two positions so far as I am able to state" (S, 7).

"A faint horizontal line moved up and down. I saw it move down jerkily and then go up very smoothly and quickly. This happened several times. I saw the line in several positions going down but nothing as it went up" (K, 9).

"Distinct impression of a slow movement downward and a quick movement up. There were diffuse pressures around the eyes" (G, 7).

"Very easy movement, slightly irregular, in a downward direction; then smooth movement up. I could follow the line with my eyes as it went down. I saw it at several places. I am not sure of anything in the upward movement except that the line moved up" (R, 9).

IV. Discussion of Results

Several studies of visually apprehended movement have been conducted within the past few years. Of these the two which stand out as most significant, both for the general theory of stroboscopic perception and for our own particular study, are the investigations of Wertheimer and Dimmick. From Wertheimer is derived in hypothetical form an interesting explanation of visual movement; an explanation which Dimmick rejects, substituting in its stead one more in harmony with the traditions of descriptive psychology.

With Exner and others, Wertheimer contends that the experience of visual movement is just as simple and as elementary as the sensations of the analyst; that movement cannot be resolved into 'simpler' constituents and still remain movement; that it is an experience which can and often does exist in and of itself. It is just movement, simple and irreducible; movement without qualitative or spatial characterizations. It may appear, it is true, along with some object; but under proper objective conditions the movement may also be completely divorced from the object and so become 'pure-phi' or 'pure movement'. For him the best movement is the 'fullest' movement. It appears near 60 σ and grows progressively poorer as one passes temporally in either direction from this optimal interval. At 30 and 200 σ FM is not to be apprehended. His explanation, as we have seen, is neural. The simple and unitary

experience of 'full' movement depends upon a simple and indivisible cortical process effected between two adjacently excited cortical regions. This is best effected at 60 σ, but not at relatively short or at relatively long temporal intervals. When this cerebral process occurs, movement appears without 'qualitative' aspects.[28]

Dimmick interpreted his results as standing against a movement of non-qualitative character; *i.e.*, movement depending solely upon central conditions and wanting a phenomenal basis in vision. He sought to show, instead, that visual movement may be closely correlated with a definite experiential factor, namely, a grey of a certain, fairly brief, durational course. A significant connection exists, then, as Dimmick thinks, between a particular interval and a particular visual configuration whereby the apprehension of a particular type of movement is conditioned. With a shift in the temporal interval there goes a change in the character of the primary integration of quality and duration. As Dimmick puts it "the integration . . . . is directly determined by the temporal interval between the exposures of the two stimulus-members" (p. 332).[29] Upon this basis was formulated an explanation of the change in the nature of visual movement under slight shifts from the 'optimal' interval. This explanation is hard to follow and to verify because Dimmick does not give the intervals at which his bimembral and his unimembral movements occurred. Since Dimmick found the shift in the 'process' configuration to be directly dependent upon the temporal interval (see his conclusions) and since any shift in the process pattern or the temporal interval meant, as he thought, a corresponding change in the type of perceived movement, it is strange that he does not cite figures or other testimony to the alleged change of optimal over into unimembral and bimembral movement as he passes temporally in either direction from the optimal interval. At no place does he show the number of cases of unimembral, bimembral and intramembral movements at various temporal intervals, *i.e.*, the relation between these movements and the time variable. It would have been illuminating to know the number of such movements actually apprehended in connection with every one of h s intervals. One is led necessarily to assume from his descriptive account that these incomplete movements did not occur except around the shorter or the longer times. So much,

[28]From this simple beginning, the doctrine has been widely extended through the works of Koffka and Köhler to include also many inanimate and inorganic events. (*Cf.*, H. Helson, The Psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62.

[29]*Cf.* also F. L. Dimmick and G. H. Scahill, Visual Perception of Movement, this JOURNAL, 36, 1925, 412-417.

at least, is intimated in the statement that "our intervals lie between those which give succession and simultaneity" (p. 322).

We have now to consider our results in the light of these two investigations. We cannot hope to compare in all details the three experiments, since our stimulus conditions and our instructions were somewhere dissimilar. It appears, nevertheless, that the results are not incomparable, inasmuch as we all obtained, under the same setting, full-movement, dual-movement, partial-movement and movement within a member; in short, all the types observed. We are justified, therefore, in a general discussion and comparison of all three investigations with respect to their common elements.

Since the correlation of a particular type of movement with a particular temporal interval appears as one of the most significant facts for both Wertheimer and Dimmick, we shall deal first with that matter. We have pointed out in our own experiments that, at no time, could we say that one, rather than another, temporal interval conditioned FM. We have seen that FM was as clearly apprehended at the extreme intervals as at 60 or 90 σ. This was an outstanding fact in all of our experiments. *O*s commented on the fact that the observations were often so essentially alike that they might, at times, have characterized a whole series (*i.e.*, 20 observations at all 10 intervals) by a simple brief description. As over against this wide extension, however, we have the generalizations of Wertheimer and Dimmick concerning the optimal interval. As widely as the latter departs from the former with respect to the experiential character of visual movement, there is, nevertheless, an agreement between them that FM does generally require a particular and definite interval.

To what shall we attribute this striking disparity between our results and those of Wertheimer and Dimmick? It is difficult to say. Perhaps it is due to a difference in experimental setting and control. We have the statement from one of our *O*s who took part also in the Cornell study, that movement was decidedly clearer and more insistent under our conditions. It might well be, were it not for the facts of Series VII, that the elimination of everything in the visual field but two bright lines on a lightless ground is more conducive to the experience of visual movement than black lines on a moderately large white expanse. In Series VII, however, where our cards of light grey with black lines were apprehended as a bright rectangle with dark figures, we were approximating the conditions of the Cornell study in all respects other than the sudden and necessary increase in the intensity of the illumination of the second figure to effect the removal of the first. To whatever the difference is due we must assert, in general, that no particular interval may be said to

give FM as against another which does not. Moreover, it does not seem probable, in the light of what we know concerning the functional operation of the organism, that visual movement should wholly depend upon any particular interval. Rather it seems to us that the human organism should be able to arrive at the same functional outcome under fairly dissimilar extra-organic conditions. Everyone knows the wide variety of conditions (visual, auditory, dermal, kinaesthetic, the displacement of objects, of the eyes, or of the body, etc.) under which the perception of movement arises. With a scarcity of one kind of resource, under one set of objective conditions, other means at hand will be utilized for this perceptual performance.

Not only are we unable to set up a significant general parallelism between temporal interval and full-movement; we find that FM may also occur under phenomenal simultaneity. This fact seems highly significant for Wertheimer's original theory. It means that, under our conditions, the shorter intervals do not give a 'resting' simultaneity of the two members but, instead, a very clear and insistent movement. This was observed, as we have said, by all *O*s including Professor Koffka. It cannot be attributed to, or explained by, an increase in the length of either exposition-time; since the same exposures which gave full-movement at 225 and 60 σ gave FM also at 15 σ. This explanation, as we know, has been previously offered; but since our exposition-times were kept constant in Series I-IV (with both A and B) it is not applicable to our facts. One wonders whether Wertheimer would have postulated his doctrine had he observed FM under simultaneity. We are unable to see how he could consistently have developed his central theory as he did, had his facts been a little different. Our results here, as we find, throw a shadow of doubt upon one of the most fundamental and basic assumptions involved in the *Gestalt-Theorie*, in so far as that doctrine assumes a spatially and temporally extended cortical process occurring under certain extra-organic conditions, the most important being the temporal interval necessary for the *phi-function*. Our facts seem sufficient to warrant generalization on this point. We have a total of 3660 reports. Three phases (V, VI, and VII) of this experiment deal with o intervals. In addition there are the times of 15, 30, and 45 σ of Series I-IV. In the light of these observations we believe that the assumption of the *phi-function* as running its course under a certain necessary temporal interval is invalid and does not stand experimental test. A modification of the physiological explanation of visual movement as postulated in the doctrine of the *Gestalt*, appears, then, to be called for.

Wertheimer's descriptive facts are not, however, without value. While the essential features of his work had been-

sented by earlier investigators, his experiments contributed significant material. We find that our own results in Series I-IV (Objects A and B) tend largely to substantiate his account of the various forms of movement. His *Os* reported rapid and slow, smooth and irregular movement from one position to another. His statements of dual-movement are in no way essentially different from ours; and the same is true of the partial-movement of a single member. Since we had few cases of movement within a member, we have less foundation for comparison there; but no striking difference appears in those which we do have. In one very important respect our results stand in close and significant agreement with those of Wertheimer. Very often our *Os* reported clear movement in connection with some part of the total configuration—around or between the members,—although they were quite unable to characterize this movement in any more precise manner. For Wertheimer it was just movement without quality and without a definite spatial character. We find the same in our own observations. Prior to our experiment we were unable to realize how visual movement could exist without a phenomenal visual basis being clearly evident. It was not long before we realized that the *Os* were apparently experiencing this very thing and finding themselves unable to give a clear account of it. Often the two members were present, with a clear impression of movement between. At other times, the lines appeared in succession but with the impression that something had moved about or over the total figure. We must agree, then, with Wertheimer that, at times, the experience of 'pure' movement taken in this sense may occur. We further agree with Wertheimer that this sort of movement occurs in conjunction with the shorter rather than with the longer intervals. In substantiating Wertheimer's description we find ourselves in general disagreement with the explanation and description of visual movement as offered by Dimmick. As we saw, Dimmick rejected Wertheimer's explanation. He found, as he thought, that visual movement had an 'existential' correlate. Where there was optimal movement, for example, the scene of the movement was—for him—filled with a "visual grey" which was itself wholly unmoving. Where there was bimembral movement the "process" accrued to each member, but not to the field as a whole. Finally, where there was unimembral movement the "process" was connected with one member only. In case of intra-membral movement, the "process" was said to lie within the member,—a description which seems a trifle vague.

Now we have to consider whether Dimmick was wholly justified in rejecting Wertheimer's claim that visual movement does not have a specific qualitative visual character; and, further-

more, we have to scrutinize the evidence for Dimmick's existential aspect of visual movement. It must be admitted that, regarded from one point of view, it would be extremely gratifying to find a simple one-to-one correlation between visual movement and some simple invariable mental quality which in itself exhibits no movement.[30] That would settle the matter. But nature does not consult our wishes. In fact, the human organism is so sophisticated that simplicity is hardly to be expected in experiences much less complex than the apprehension of apparent movement under discrete retinal stimulations. Our *O*s, who observed without knowledge, reported insistent movement from o interval to 225 σ, time after time, without being able to report any relevant existential visual correlate between the lines. All experienced movement and described its rate, extent, regularity and continuity. When questioned upon the visual character of the interlinear field, they usually reported that there was nothing to distinguish that region from the general visual ground. The description implied then no significant visual relation to the experience of movement. Most of these *O*s were wholly uninformed about the current discussion of visual movement. Moreover, it is to be recalled that, of our *O*s, one served in the investigation at Cornell and was well acquainted with the meaning of the report made in terms of existential qualities. From this *O*, as well as from all the others, we have various reports of full-movement where the field was described as unfilled by any quality different from that of the background. To what shall we attribute this difference between the Cornell study and our own? Let us consider certain internal aspects of Dimmick's account. We must examine certain of his 'movement' reports which seem to us ambiguous. A different interpretation and classification might easily and properly be drawn from them. This ambiguity is, as we think, very important. The first 9 of the following reports are cited in the Cornell study as unimembral movement; the last 2 as bimembral. They are taken from pages 325-326.

(*1*) H. "An oblique came in and moved downward, and below it stood a horizontal line."

(*2*) S. "A line moved down smoothly, getting faster as it went, and then the end-position stood out, sharp and stationary."

(*3*) M. "A horizontal line came into the part of the field moving, then another appeared lower down and stationary."

(*4*) Tu. "First I saw a line which wavered but did not change position, and at the same time there was something moving down into a position below."

(*5*) Ta. "A line moved down, and as it moved another appeared lower down and stationary."

(*6*) M. "An oblique moved down toward the horizontal. The movement was jerky and definitely of the oblique, not of the horizontal."

[30] "There is, then, no movement in the multiform grey flash; the space is constant; the integration is of time and quality" (p. 332).

(*7*) S. "I saw a line move down very rapidly through an arc. The first part of the movement was not clear, *i.e.*, it seemed to be part way down before I saw it; above was a blank."
(*8*) Ta. "An oblique line appeared, then a horizontal and the oblique moved down to the horizontal."
(*9*) Tu. "An oblique line appeared in the upper part of the field and moved down faster and faster until I could not see the movement. Then the horizontal came in."
(*10*) S. "An oblique line moved down to the horizontal, but there was a pause in the movement part-way down."
(*11*) Tu. "An oblique line moved down to the horizontal but not smoothly; there was a flash of good movement at the beginning and end."

According to (*1*), the movement might just as well be considered full-movement as unimembral. *O* reported two lines, one above the other, with movement between them. In (*2*) report we find even greater uncertainty. Was movement over the whole field? So we are to believe from the term "end-position" in connection with movement. Our uncertainty is further increased by the report of this *O* for *optimal* movement (p. 323). "A rectangle moved down smoothly and stopped for an instant. The speed of the line increased as it moved, and was very rapid just before it stopped." The two reports are very similar. Either the report for optimal movement is in the wrong place or the report given as an example of unimembral is questionable. An unbiased reader might easily consider the second instance above as full-movement. The report of (*3*) is much like that of (*1*). It is difficult to determine from the character of the report whether movement is unimembral or full. The same criticism holds true for the next case (*4*). There might have been full-movement of the second member with the first retaining its position. We had ourselves, as we have indicated, many cases of full-movement where the meaning was that the second line moved away from the first to its own final position. The report of Ta (*5*) is like that of H (*1*). In (*6*), movement might well have been over the entire field, so far as we may judge from the report. Case (*7*) appears as full-movement; since movement was from one position to another; *i.e.*, above the place where the movement started, the field was, according to *O*, a blank. This is what is meant by full-movement. Case (*8*) is the least ambiguous of all so far. What puzzles us is how or why Dimmick considered this to be partial movement of one line; when *O* said the oblique moved all the way across the field. In case (*9*) there is, again, an ambiguity. If the horizontal came at the lower end of the movement, then it is full movement; if it came below with a break between it and the movement, then it is partial; but as the report stands we wonder how Dimmick could determine the exact character of the experience. There is just as much ground for calling it full-movement as for claiming it to be unimembral. In (*10*), once more, we are wholly unable to understand why the report should have been called bimembral. Surely, a pause, as at a line, is not a break; and without a break in continuity of movement it could not be considered as bimembral. The same criticism holds for case (*11*), which might quite properly be considered as full-movement. *O* does not state the movement was broken;—it was simply not "smooth." But smoothness and continuity are surely not identical.

There are still more instances, in Dimmick's protocols, of the same general character; but these are sufficient to exemplify our contention that the reports are subject to an interpretation different from that placed upon them in the Cornell study. We took 9 cases of 16 given as illustrative of unimembral movement. One is forced to ask why, when there were, presumably, so many cases of true unimembral movement, 16 perfectly

straightforward and unambiguous cases were not given. Or why movements which might very properly be considered as optimal should be cited as bimembral.

This suggests a further uncertainty. As we know, Dimmick stands for a close correlation between "process" configuration and type of movement. But when we turn to his reports we do not find the evidence clear-cut. I cite several "meaning" reports together with corresponding "process" reports taken from pages 325-328.

Meaning	*Process*
(*1*) M. "A horizontal line came into the part of the field moving, then another appeared lower down and stationary."	"First came a line in the upper part, then a grey, and another line below. The grey joined the lower line but was separated from the upper by a bright streak."
(*2*) S. "A line moved down smoothly, getting faster as it went, and then the end-position stood out, sharp and stationary."	"I saw a horizontal line, then lower down another with a grey patch above it."
(*3*) Ta. "An oblique line appeared, then a horizontal, and the oblique moved down to the horizontal."	"The oblique was grey on its lower side and blurred downward. There was a white space between it and the horizontal line."
(*4*) W. "An oblique line moved rapidly to the horizontal. The movement was best at the beginning, and just at the bottom there was a space of no movement at all."	"A distinct oblique line appeared at the top, then a horizontal, and between them a series of grey triangles, light at the top and dark at the bottom and joined on to the lower line."
(*5*) S. "An oblique line moved down to the horizontal, but there was a pause in the movement part-way down."	"First there appeared an oblique line, which was not clear at the lower edge but shaded off into grey. Below there was an interval of white, and at the bottom a horizontal which also lacked definition."
(*6*) C. "An oblique line moved down to the horizontal. The movement was not smooth; it disappeared in the middle so that there was a blank space."	"I saw an oblique line. It disappeared and the field was only white, then a horizontal appeared. Both lines were indistinct and blurred toward the interior of the angle."
(*7*) D. "I saw two lines, one oblique and one horizontal. The latter remained stationary, and the oblique moved down toward it, but was gone before reaching it."	"A sharply defined oblique line appeared and then a horizontal. When the horizontal came, the field just above it was greyish."
(*8*) H. "An oblique line came in and moved downward, and below it stood a horizontal line."	"First there was an oblique line with a patch of bright field just below it; then at the bottom a narrow sector of light grey, which was later replaced by a horizontal."

Consider these cases. We are led to assume that where there is movement there will likewise be the "psychological correlate" (D's visual grey); that some visual process "will be found closely paralleling the . . . perceptions" (p. 321). Where the field is filled, there will be full movement; where the process accrues to one member, movement will be unimembral; and where the process involves both members, movement will be bimembral. It must be understood, of course, that, according to the method employed in the Cornell study, the two reports were not taken from the same identical exposition. Under one exposition *O* reported upon movement and under another exposition and another instruction, upon 'quality.' These reports are cited by Dimmick as representative of the alleged correlation.

In (*1*) we have unimembral movement. The process accrues to the *second* member; but the *first* moves. In (*2*), the grey patch is above the *lower* line; but the *upper* line (at least) moved. (*3*) appears to be *full-movement*, but with the grey accruing only to the *upper* line. The field is *filled* with "grey triangles" in (*4*); but only *partial* movement of one line was experienced. (*5*) might be *full-movement*, but with a *part* of the field *unfilled* with grey. In (*6*) there was *nothing* in the field of a phenomenal character save the two lines, yet there was *movement* of both. The grey accrued to the *horizontal* in (*7*); but instead the *oblique* moved. (*8*) is much the same; although the *horizontal* (not the oblique) had the accompanying grey, it was the *oblique* which moved.

Now our criticism is this. If Dimmick meant these reports of 'meaning' and 'process' to be taken together, we can understand his intent; but so taken they seem well-nigh meaningless. There is no significant parallelism between them. If he did not mean the two sorts to be considered together, why should he place *process* reports under such captions as *optimal, unimembral* and *bimembral* movement, in this way indicating them as illustrative of the process side of these specified perceptions? For he tells us that "the nature of the visual experience which filled the interval can be pointed out most clearly by direct reference to the introspections" (p. 323). How did he know, we must ask, that a particular process configuration which he placed under unimembral movement really belonged there rather than under some other head? Or why did he put down as the process-configuration for bimembral movement two lines each slightly blurred at one end? A more obvious illustration of this point is found in his account of pure *phi*. Both lines might be clearly perceived with movement between them; or neither line was perceived—only movement. In the first case, under the process attitude, *O*s reported the total configuration; in the second, merely the flash without the limiting lines. How was Dimmick to know one total configuration from another? Or what (unreported) difference existed on the side of process to give, on the one hand, optimal movement of a single line, and, on the other, bare but still optimal movement? To have designated certain

process-reports as 'optimal', 'bimembral', or 'unimembral', *either O* noted and reported the movement and its precise character under process-instruction, *or* the character of the movement which might have been observed under the perceptive attitude is inferred in the light of Dimmick's hypothesis of parallelism from the process reports. The reader is not told which procedure was followed.

Dimmick offers, as additional proof of the parallelism, the fact that "in a number of reports the *O*s have stated that a certain movement had a particular process-aspect and conversely that . . .," (p. 328). If, under the process-instruction, *O*s generally observed only "process", there would seem to be neither logical nor experiential justification for claiming that the process configuration could possibly mean anything more than just what it was. It could scarcely be set up as the basis for a particular movement-experience. What the organism apprehends at one moment is not to be identified with its earlier and later experiences. To contend, *e.g.*, that, because S (Table I, page 321) observed optimal movement 72 times, unimembral 23 times, and bimembral 7 times out of 100 observations,[31] under movement-instruction; while, under process-instruction, the same *O* reported 1 sort of configuration 90 times, and 2 other kinds 1 time each, there is a significant and thoroughgoing connection between the two sets of observations is hardly logical. If we assume, however, that movement was apprehended just as clearly and as insistently under the process as under the perceptual instruction, the observations approach those of Wertheimer and of our own *O*s.[32]

The numerical discrepancies in the parallelism of the two sorts of reports should not be overlooked. The figures below were obtained from Dimmick's Table III (designed to show the influence of the temporal interval upon *optimal* movement with the horizontal lines) by taking the difference between P and M. The "plus" sign represents the percent. excess of process over meaning reports; and the "minus" sign, the excess of meaning over process. At 30 σ there were 103 more cases of full-move-

30 σ	60 σ	90 σ	120 σ	150 σ
+12	− 3	+ 2	+ 3	− 7
−25	+25	− 5	+35	− 5
+ 5	+15	+ 5	+ 5	+20
−25	−17	+15	0	+62
−23	− 2	+ 3	+53	+12
−20	+28	+27	+ 7	+15
+ 5	+ 5	+15	− 5	+18
−10	− 5	+ 7	+33	+ 8

[31]The addition is Dimmick's, not mine!

[32]In fact, in a more recent study on Visual Movement, Dimmick uses only the *process*-instruction, this JOURNAL, 36, 1925, 412-417.

ment than of corresponding process-configuration (5 *O*s); at 90 σ 74 more cases of total configuration than of movement (7 *O*s); and at 150 σ the field was filled with grey 135 more times than movement was reported (6 *O*s).

Again, we may ask how Dimmick proposes to account for differences in rate of movement within the visual field under a given exposition? For example, his *O*s reported movement as increasing or decreasing in rate toward the end. If the field were filled *uniformly* with a grey (to condition full-movement) or *partially* (to give partial-movement), we should expect movement to be the same in character throughout the particular exposition unless the 'process' report indicated a precise phenomenal basis for the observed difference in rate. But an inspection of the actual (process) reports does not show, in a single case, any ground for this difference. The reader gains the impression that a plain but brief grey was being described; a grey such as is sometimes secured in tachistoscopic expositions. Only near the end of the study is the reader informed that the visual process was not, at any time, a "text-book grey." Beyond the figurative terms "flashing," "flickering," and so on, we have little or no description of the temporal aspect, which is presumably, for the author, as important as the qualitative.

What shall we say at large of the gray intermediate to the lines? In our own observations it is neither a constant phenomenon nor one necessary to the perception of movement. The intermediate filling is, when it occurs, one among several visual and non-visual resources which may or may not be utilized by the organism at a given moment in the apprehension of movement.

In the addition and the subtraction of figures, we described movement under conditions which seem to us to be new. Here the *O*s reported 'easy' and unimpeded movement over the entire field from one line to another. What light do these Series (V and VI) throw upon the problem of movement under the method of successive presentations with interval? There an object is apprehended, followed by a second coming out of the first; or else the first moves over to form the second. But here movement depends in part upon the apprehended relation between the members. If the lines are seen as unrelated or are perceptually unconnected, there is no movement. The members must be ligated to condition movement. In these Series, which we consider as an extension of the method of successive presentations, we sought to determine the principle of ligation.

How is the movement in these series to be explained? We indicated a possibility. Eye-movements certainly occur, with addition or subtraction of members, at the precise moment of visual movement. There is, moreover, the fact of the negatively

colored lines moving across the field, as well as the momentary disappearance of the fully exposed line when movement is seen. There is, finally, the matter of the visual intermediates. In terms of eye-movements we can most adequately explain the apprehension of the lines as successive positions. In addition to these items there was, as we have indicated, a visual basis, at times, for the movement. When one line was added, the field might be partially or wholly filled by a dim light which spread out from the lines. In subtraction, this filling, when it was present, had to go. *O* occasionally mentioned that the disappearance of the line was accompanied by a clear 'swooping up' of part of the light. Obviously, it is difficult to account in purely visual terms for FM under this last condition. *Gamma*-movement might be conditioned by the phenomenal shrinkage of the field; but FM in wholly visual terms in quite different. FM where retraction is in a direction opposed to movement perceived is a visual paradox.

What we have said about the qualitative aspect of visual movement furnishes abundant proof of a fact which struck us as being particularly impressive. We have here clearly demonstrated the remarkable facility of the organism whereby it may attain a functional end under extreme poverty of experiential resources. At times, one is hard-pressed to indicate the experiential items involved. The phenomenal basis is often so vaporous and tenuous that one is driven either to central conditions or to obscure somatic factors. The *O*s noticed this inadequacy of 'tangible' experiential factors. They were certain of movement but not of the means utilized to apprehend it. Their reports indicate that the perception progressively grew thinner, especially toward the close of a sitting. It was as if the organism became definitely inclined toward an immediate apprehension. Under such conditions use was made of any kind of experiential material at hand. But the material was often so obscure that *O* could only hint at its character.

Apprehension of movement stroboscopically appears, on the whole, to resolve itself into functional operation under various central, peripheral and extra-organic conditions. The relative contribution of each varies from occasion to occasion. We saw that certain objective conditions (*e.g.*, rate and interval) might be widely altered without producing significant differences in perception. We find no reason for believing that any single quality (such as a 'flashing grey') may be set up as the experiential basis of visual movement. We saw that so long as one member is held over or related to another, or so long as one member is apprehended in two or more successive positions, movement ensues. This ligation may be accomplished by a variety of experiential and somatic factors. But movement may

also depend, as we believe, upon the operation of a firmly entrenched set, capable of producing, even when released under extremely diverse objective conditions, the same functional outcome. Many of the cases where the experimental basis was meager could be attributed, so we surmise, to a highly habituated perceptive attitude. The progressive 'bareness' of the performance may be partly ascribed to this central disposition. Under the extreme limits of functioning it means, as Linke found, that strict object-identification is not necessary—the organism using merely a *'that-that'* or a *'there-there.'* In this way, triangles, for example, may move into circles, or movement may be seen before the object is perceived.

Besides the influence of central factors, there were, we said, other factors for connecting movement-wise the discrete expositions. In the absence of all visual items other than the perception of successive positions, clear movement was apprehended through kinaesthesis. These experiential qualities served to integrate the whole perceptual incorporation; that is to say, to join the otherwise unrelated visual constituents. Verbally stressed exposures, ocular shifts and lingual pursuit movements, as well as slight or incipient rhythmic or non-rhythmic movement of the general musculature furnished an experiential background for the purely visual items to functionate in perception.

The contribution of central tendencies and kinaesthesis to the apprehension of movement includes many factors of a non-visual character. In them we find justification for the assertion that the visual perception of movement may be considered as an abstractive perception in the sense that one need not see object of particular dimensions as moving. It may mean only that within a given area of apprehended space something has moved. There is vague spatial characterization but little shape or filling. The relative degree of abstraction depends primarily upon the relative contribution to the performance of central factors and kinaesthesis as against the contribution of purely visual moments.

V. Conclusions

Many experiments have been conducted upon the problem of visually apprehended movement under discrete retinal excitations. Various explanations have been offered; but no single explanation that is wholly adequate to the facts has yet been advanced. Two have been presented within recent years;—the one given by Wertheimer is physiological, the other offered by Dimmick is psychological in character. In this investigation we have sought to test the bearing of the facts upon these rival explanations. Our results justify, as we believe, the following conclusions.

(*1*) It is impossible to set up a significant correlation between any one temporal interval and full-movement.

(*2*) Clear and insistent visual movement may occur under phenomenal simultaneity of both figures.

(*3*) Pronounced visual movement may be objectively determined by the immediate addition (without interval) of one member to another to form the acute angle or the two parallel lines.

(*4*) Clear movement may be likewise obtained by the sudden removal, instead of the addition, of one member.

(*5*) Insistent visual movement may be apprehended under successive presentations with no temporal interval between.

(*6*) Wertheimer was correct in his description of certain visual movements which do not lend themselves to a characterization in qualitative terms of vision.

(*7*) The fundamental assumption of Wertheimer's physiological explanation: namely, that the *phi-function* requires a determined and specific temporal course, does not stand experimental test. It must therefore be abandoned.

(*8*) The significance of a visual grey of a certain durational course, alleged to be the psychological correlate of visual movement, appears, in the light of our experiments to have been exaggerated.

(*9*) Non-visual resources are utilized to a greater extent in the apprehension of movement than Wertheimer and Dimmick recognized.

(*10*) The visual apprehension of movement under discrete retinal stimulation is a form of abstractive perception determined by stimulus, receptor and central organs, the degree of abstraction depending upon the availability of experiential items and upon the relative contributions of environmental and of organic factors.

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THE EFFECT OF REPETITION ON ENDING PREFERENCES IN MELODIES[1]

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HISTORY

Besides the innate factor in musical aesthetics, there is the habit effect. This has been shown in the work of Moore[2] and others.

Moore gives, in his monograph, a resumé of interval preferences, showing the varying ratings in the several historical periods. Thus, in ancient times, the octave, fifth and fourth were considered the most pleasing intervals, the thirds and sixths displeasing, and the seconds and sevenths extremely discordant. In our day, however, the first three named are classified as pleasing but uninteresting, the next two as very pleasing, and the last mentioned as still somewhat displeasing but of increasing interest.

Experiments performed by Valentine[3] on 146 adults, all university students, give similar results. The order of preference from best to worst was; major third, minor third, octave, major sixth, minor sixth, fourth, tritone, fifth, major second, minor seventh, major seventh, and last the minor second.

This historical evidence is strengthened by the experiments which Moore also undertook. *Ss* were trained by having certain intervals presented to them many times. The change in the preference ratings which followed was then tabulated. The results showed that the interval preferences of individuals can be enormously modified through training.

INTRODUCTION

Meyer[4] has shown that, other factors constant, a melody which ends on the ratio symbol 2 is preferred to all other melodies.[5] This fact constitutes the tonic effect in music. In a previous paper,[6] the writer demonstrated that atonic endings —those not ending on ratio symbol 2—differ among themselves in preference ratings. That is, where the tone c is ratio symbol

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[1]From the Department of Psychology of Ohio State University.

[2]H. T. Moore, The Genetic Aspect of Consonance and Dissonance, *Psych. Mon.* 17, 1914, No. 2.

[3]C. W. Valentine, The Aesthetic Appreciation of Musical Intervals Among School Children and Adults, *Brit. J. Psych.*, 6, 1913, 190-216.

[4]M. F. Meyer, this JOURNAL, 14, 1903, 459.

[5]A ratio symbol is the frequency ratio with the pure powers of 2 extracted. For example, c is to f as 3 is to 4. The ratio symbols would be c, 3; f, 2.

[6]P. R. Farnsworth, Atonic Endings In Melodies, this JOURNAL, 36, 1925, 394-400.

2, a melody which ends on g will be preferred over one ending on e, and e over b♭. As the ratio symbols of c, g, e, and b♭ are 2, 3, 5, and 7 respectively, this means that, as an ending, 3 is preferred over 5 and 5 is preferred over 7.

Part 4 of the summary of that paper states, "When compared with ratio symbols 5 and 7, 3 apparently gives more finality than 5, and 5 more than 7, in the proportion of: symbol 3, 41%; symbol 5, 34%; and symbol 7, 25%."

Problem

The problem then arose to determine the stability of these preferences. For example, might the percent. be changed in the direction of an increasing preference for the ratio symbol 7 as an ending over ratio symbol 3? Experiments were undertaken to ascertain the answer to the above and similar questions.

The 30 Judgments Test. The manner of approach was in part similar to that of the previous problem. 3 tones,—g, e, and b♭,—were chosen as representing the ratio symbols 3, 5, and 7, where c is ratio symbol 2. The corresponding musical notation, vibration-ratios, and actual vibration-rates, are here listed.

Musical Notation	*Ratio Symbol*	*Vib. Ratio*	*Vib. Rate*
g	3	6	96.89 d.v.
g′	3	12	193.77 d.v.
e′	5	10	162.94 d.v.
b♭	7	7	115.22 d.v.
b♭′	7	14	230.43 d.v.

These tones were played successively on a reed-organ. The 3 tones were so arranged that in one group g (3) was the last tone heard, in a second group e (5) was the last, and in a third group b♭ (7) was the final tone given.[7] The groups were sounded in a random order, and judged by the method of paired comparisons, as shown in the following experimental programme.

Thirty judgments were made by every *S*, as follows:
- Ten judgments with 3 and 5 paired
  - Five judgments with 3 first and 5 last
  - Five judgments with 5 first and 3 last
- Ten judgments with 3 and 7 paired
  - Five judgments with 3 first and 7 last
  - Five judgments with 7 first and 3 last
- Ten judgments with 5 and 7 paired
  - Five judgments with 5 first and 7 last
  - Five judgments with 7 first and 5 last

Instructions. The instructions were as follows: "You will hear two series of three tones each, which you are to compare. Which series sounds more finished, more complete, more at rest? If it is the first, mark '1' on your paper; if it is the second, mark '2'." The instructions were repeated so that every *S* understood what was expected of him.

[7]The numbers in parentheses in the text refer to ratio symbols.

Subjects. 3 groups of *Ss* were selected from the summer school student body. They will be designated groups A, B, and C. Group A averaged 12 members, group B 9, and group C 8. Each group met for approximately a half hour every Monday, Wednesday and Friday, until groups A and B had each completed 7 work periods, and group C 11. The end of the summer quarter was then at hand. Group C's greater number of work periods is due to the fact that it started some days earlier, and held to its work on days when vacations interfered with groups A and B.

Procedure. At the beginning and end of every work period, the 30 judgments test, described above, was given. Between these, a drill period was inserted which consisted of listening to a prepared series of tone combinations. These drill periods differed in some respects with the several groups. The effect of this drill period was measured by the differences between the beginning and end tests.[8]

Experiments and Results

Group A. The drill for this group was in hearing the melody end on ratio symbol 3. During the drill period, the *Ss* heard tones in the following order,—a key note (ratio symbol 2), pause, ratio symbol 5, 7, and last 3. Examples would be: c, pause, e, b♭, g; g, pause, b, f, d; etc. These were all played with a falling inflection. Such sequences were played, using the different scale notes as keys, and proceeding up and down the scale. It should be noted that the drill melody invariably ended on ratio symbol 3. This drill was continued slowly for some 10 min. During this time about 70 sequences were presented.

Example of Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	e′	5	10
2nd.	b♭	7	7
3rd.	g	3	6

After this, the 30 judgments test was again given. With group A, the tones in this test were heard in a rising, then falling inflection. An example would be: e (5), the next higher b♭ (7), then the next lower g (3). The percent. of preferences for ending the melody on g (3) was then calculated, and compared with that found at the beginning of the hour.

Example of Rising then Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	e′	5	10
2nd.	b♭′	7	14
3rd.	g′	3	12

[8]For more detailed explanation of the technique, see the writer's article referred to above.

Percent. of Preferences for g (3)

Period	Beg. of Hour			End of Hour			Inc. during Hour			Inc. over 1st Day		
	g	e	b_b	g	e	b_b	g	e	b_b	g	e	b_b
1	38	33	29	42	36	22	+ 4	+ 3	− 7	+ 4	+3	− 7
2	34	36	30	44	36	20	+10	0	−10	+ 6	+3	− 9
3	43	42	15	48	39	13	+ 5	− 3	− 2	+10	+6	−16
4	48	42	10	53	38	9	+ 5	− 4	− 1	+15	+5	−20
5	45	43	12	55	39	6	+10	− 4	− 6	+17	+6	−23
6	48	40	12	48	40	12	0	0	0	+10	+7	−17
7	42	45	13	55	35	10	+13	−10	− 3	+17	+2	−19
Average daily increase							+ 7	− 3	− 4			
Inc. from beginning to end							38 to 55			+45		
							33 to 35				+6	
							29 to 10					−65

Conclusion: a drill of 10 min. on ratio symbol 3 gave an average increase of 7% in the preferences, or an increase of 45% from the beginning to the end of the experiment.[9]

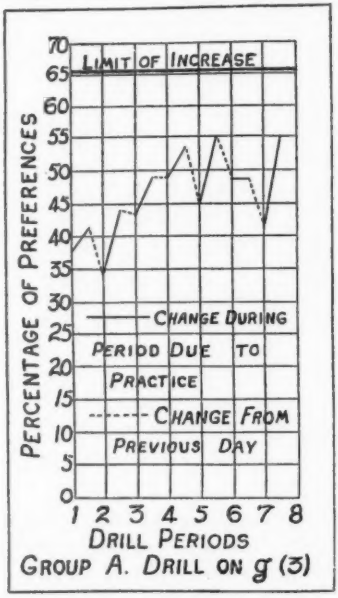
LIMIT OF INCREASE
PERCENTAGE OF PREFERENCES
CHANGE DURING PERIOD DUE TO PRACTICE
CHANGE FROM PREVIOUS DAY
DRILL PERIODS
GROUP A. DRILL ON g (3)

FIG. I

Explanation of Fig. I. When 3 factors are being compared by the method of paired comparisons, 1 factor appears in only ⅔ of the ballots, so that it can maximally receive only ⅔ of the

[9]Nine controls on this and subsequent groups showed slight variations.

votes. With preferences expressed for the 3 ratio symbols, the highest score possible for any one of these would be 66%. This percent. is therefore labelled 'Limit of Increase.'

From the solid lines, it can be seen that the preferences increased after drill in almost every instance. The downward slope of the majority of the dotted lines shows loss of training from one practice period to the next. That the curve is not smooth is to be expected in view of the small number of subjects and the daily variations—metabolic and otherwise.

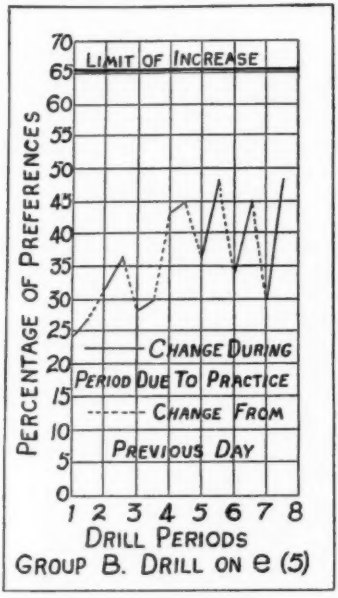

FIG. II

Group B. The drill here was for ending on the ratio symbol 5. In the 30 judgment test a falling inflection was employed—as g (3), next lower e (5), next lower b♭ (7).

Example of Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	g′	3	12
2nd.	e′	5	10
3rd.	b♭	7	7

During the drill period, the *S*s heard tones in the following order—a key note, pause, ratio symbol 7, 3, and last 5. Ex-

amples would be: c, pause, b♭, g, e; g, pause, f, d, b; etc. The inflection here was rising, then falling; as b♭, next higher g, next lower e.

Example of Rising then Falling Inflection

Order Presented	*Mus. Notation*	*Ratio Symbol*	*Vib. Ratio*
1st.	$b_\flat$	7	7
2nd.	g′	3	12
3rd.	e′	5	10

Percent. of Preferences for e (5)

Period	*Beg. of Hour*			*End of Hour*			*Inc. during Hour*			*Inc. over 1st Day*		
	e	g	$b_\flat$	e	g	$b_\flat$	e	g	$b_\flat$	e	g	$b_\flat$
1	24	47	29	27	50	23	+ 3	+ 3	− 6	+ 3	+ 3	− 6
2	32	35	33	37	32	31	+ 5	− 3	− 2	+13	−15	+ 2
3	28	29	43	30	38	32	+ 2	+ 9	−11	+ 6	− 9	+ 3
4	43	30	27	45	40	15	+ 2	+10	−12	+21	− 7	−14
5	37	35	28	48	30	22	+11	− 5	− 6	+24	−17	− 7
6	34	36	30	45	28	27	+11	− 8	− 3	+21	−19	− 2
7	30	40	30	47	29	24	+17	−11	− 6	+23	−18	− 5
Average daily increase							+7½	− 1	−6½			
Inc. from beginning to end							24 to 47			+96		
							47 to 29				−38	
							29 to 24					−17

Conclusion: a drill of 10 min. on ratio symbol 5 gave an average increase of 7½% in the preferences. At the end of 7 drills the percent. preference had almost doubled,—96%.

Group C. This group heard as endings the ratio symbol 7. As with group B, a falling inflection was used in the 30 judgment-test. During the drill period, the simple melody g (3), e (5), b♭ (7) was played with a rising, then falling inflection.

Rising then Falling Inflection

Order Presented	*Mus. Notation*	*Rat. Symbol*	*Vib. Ratio*
1st.	g′	3	6
2nd.	e′	5	10
3rd.	$b_\flat$	7	7

Percent. of Preferences for b♭ (7)

Period	*Beg. of Hour*			*End of Hour*			*Inc. during Hour*			*Inc. over 1st Day*		
	$b_\flat$	g	e	$b_\flat$	g	e	$b_\flat$	g	e	$b_\flat$	g	e
1	18	49	33	28	46	26	+10	− 3	− 7	+10	− 3	− 7
2	24	55	21	26	55	19	+ 2	0	− 2	+ 8	+ 6	−14
3	24	52	24	33	52	15	+ 9	0	− 9	+15	+ 3	−18
4	29	59	12	34	59	7	+ 5	0	− 5	+16	+10	−26
5	37	28	35	37	29	34	0	+ 1	− 1	+19	−20	+ 1
6	40	36	24	43	32	25	+ 3	− 4	+ 1	+25	−17	− 8
7	33	37	30	44	28	28	+11	− 9	− 2	+26	−21	− 5
8	39	32	29	41	35	24	+ 2	+ 3	− 5	+23	−14	− 9
9	33	32	35	47	22	31	+14	−10	− 4	+29	−27	− 2
10	48	27	25	48	24	28	0	− 3	+ 3	+30	−25	− 5
11	51	23	26	53	23	24	+ 2	0	− 2	+35	−26	− 9
Average daily increase							+ 5	− 2	− 3			
Inc. from beginning to end							18 to 53			+195		
							49 to 23				−53	
							33 to 24					−27

Conclusion: a drill of 10 min. on ratio symbol 7 gave an average increase of 5%, but an increase of 195% from the beginning to the end of the experiment.

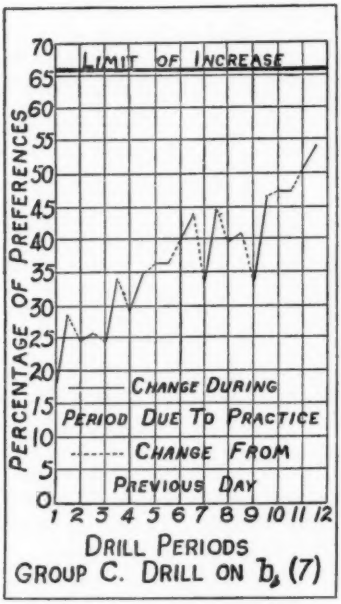

FIG. III

It is interesting to note that at their origins the curves had preferences in the order, ratio symbol 3, 38%; 5, 24 %; 7, 18%. At the end the preferences were in the order, ratio symbol 3, 55%; 5, 47%; 7, 53%.

Summary

(1) Ending preferences may be permanently altered by training. An increasing familiarity with a ratio symbol ending leads to increasing preferences for that specific ending.

(2) The type of training,—order, specific notes played, etc.,—does not seem to have a large effect. The important factor in the drill appears to be the hearing of the same continuous ratio symbol as the melody ending.

BLACK AND GREY IN VISUAL THEORY

By Gilbert J. Rich

Everyone who has given attention to theories of vision is aware that, with the exception of Hering's theory, little attempt is made to explain the sensation of black. Ladd-Franklin[1] claims that black may be accounted for on her theory, without going into the details of the explanation. And now Neifeld[2] comes forward with the promised account. He holds that the sensation of black is a sensation of constant intensity attached to a cortical situation of inactivity or rest. Sensations of grey are made up of this constant sensation of blackness and a variable amount of the sensation of whiteness. Moreover, he attempts to explain why such a constant sensation corresponding to cortical inactivity should exist. We are constantly aware of the visual field in which we live; indeed, since every point has its 'place-coefficient,' we are aware of every point in the visual field. As a result, any break in the field due to absence of stimulation would be decidedly unpleasant; and what would otherwise be such gaps are filled with a definite non-light sensation, blackness. Two questions may be asked concerning this theory of black. (1) Does it actually explain black in the sense in which colors are explained by theories of color vision? (2) Does it explain the various visual phenomena into which black enters?

In considering the first question we must remember that any sensory theory attempts to give an hypothetical account of the nervous activity involved in the process of sensing which will explain as many as possible of the facts of sensation. Thus, in the case of color theories, Hering postulates his three reversible photo-chemical reactions and explains contrast, adaptation, after-image, etc., in terms of them. Ladd-Franklin similarly explains these phenomena in terms of her genetically conceived color molecule. One may look in vain for any such account in Neifeld's paper. No nervous mechanism is postulated. We are told that black occurs when there is a situation of inactivity in the cortex, but we are not told how it occurs. That the sensation of blackness is experienced when no light waves reach the retina is well-established fact, not theory. Neifeld merely says that under these conditions we see black. The only positive aspect of this explanation is that it makes black the point of retinal inactivity, instead of the grey of the Hering-Müller hy-

[1]C. Ladd-Franklin, Tetrachromatic Vision and the Development Theory of Vision, *Science*, 55, 1922, 560.

[2]M. R. Neifeld, The Ladd-Franklin Theory of the Black Sensation, *Psych. Rev.*, 31, 1924, 498-502.

pothesis. This, as we shall see, is the crucial point of the whole argument. Does a constant black or a constant grey better explain the facts of vision? This problem leads to a consideration of the second question stated above.

In discussing the extent to which the facts of vision may be explained by the one or the other of these postulates, we may well confine ourselves, for the sake of brevity, to the outstanding facts of contrast, adaptation, negative after-image and the black-grey-white series. In the case of contrast, a positive explanation may be given in several ways. So long as grey is the center of the system, the equilibrium point of a reversible reaction or of a split color molecule, black-white contrast stands upon the same plane as does color contrast and may be explained by the same mechanisms. For the Hering theory it presents few problems once the Müllerian constant grey is postulated. The Ladd-Franklin theory might, in much the same way, postulate an original grey molecule which splits into white and black just as the white molecule splits to give the colors. This type of postulate would give a positive black sensation and a common basis for color contrast and brightness contrast. But Neifeld's theory neither brings the two forms of contrast under the same category nor affords a basis for explaining black-white contrast. If black is a constant sensation, how can neighboring stimulation by light increase it? Suppression of the so-called endogenous retinal processes cannot be invoked, since they are of the same nature as a central grey, only in the retina instead of in the cortex, against which Neifeld takes a definite stand. When the contrast is in the opposite direction, the situation is even more difficult to explain. A dark grey will produce a contrasting whiteness. Yet the dark grey is a mixture of the constant black with a low intensity of white. Now, if the constant black and a slight white excitation make surrounding objects white, why should not a mixture of the constant black and a low intensity of yellow make the surroundings yellow? But we know that this does not happen. Thus, constant black and low-intensity white have a white contrast effect while constant black and low-intensity yellow have a blue contrast effect. The difficulty is that one pole of the colorless series instead of its middle is made the neutral point. Black and white, although they contrast, are not admitted as a disappearing color pair.

White should, if the Neifeld hypothesis be valid, in time adapt to black. If the principles of color adaptation of the Ladd-Franklin theory are applied to the complete color molecule we should not expect less. If the blue or yellow part of the molecule ceases to act under continued stimulation, why should not the entire molecule behave similarly? Again, the faint grey due to the endogenous retinal processes is no help. If it is sufficiently intense to be the grey resulting from adaptation to white, it is sufficiently intense to be the constant visual

sensation, and we are back to Müller's hypothesis, only with localisation in the retina. A further difficulty in that case arises from the fact that it would become necessary to explain vision under conditions of a constant mixture of central black and retinal grey to which is added any reaction of the color molecule. Adaptation to black is likewise difficult to explain. Lack of light gives a black sensation only at first; the black changes in time to a neutral grey. Where does this grey come from? If from a constant retinal process, why add a constant cortical black? In the case of negative after-images, the arguments already set forth may be applied, since the conditions are analogous to those of contrast. Constant black added to weak white excitations results in a white after-image, while a blue after-image follows if it is added to weak yellow excitations. All of these difficulties occur because black and white are not considered either antagonists or molecular complements.

Neifeld makes no provision in his system for grey as a separate quality. It has, of course, been long accepted among psychologists that the black-white series is a single linear series and that all greys are merely admixtures of black and white. This view has been based in large part upon the supposition that, as one proceeded along the series from dark to light, the first sensations beyond the blackest black are noticeably whitish, becoming more and more so as one passes up the scale; and that, although they become less and less blackish, all colorless sensations save the most intense white have some black in their composition. More recently, however, the notion of the black-white series as forming a single straight line has been challenged, together with the observations upon which it is founded. Dimmick[3] holds that the black-grey-white series actually consists of two series which join each other at an angle of 180°. One of these series runs from black through the darker greys to middle grey; the other runs from white to middle grey through the light greys. Dimmick contends that a light grey, although resembling both white and grey, contains not the least hint or trace of black. Similarly a dark grey resembles both grey and black, but not white. Dimmick sought to illustrate the relation of grey to black and white by determining (by the method of limits) the thresholds of whiteness, blackness, redness, greenness, blueness and yellowness as a white, black or colored paper is added to grey upon a color mixer. He found that the average limens were all of the same order of magnitude and suggests that all may be regarded as rough chromatic limens, those for black and white as well as those for the four principal colors.

As a proof of his view as to the nature of the black-grey-white series, Dimmick's experiment fails to carry conviction.

[3]F. L. Dimmick, A Note on the series of Blacks, Greys, and Whites, this JOURNAL, 31, 1920, 301-302.

Similar results might be obtained even if the older notion of grey as intermediate between black and white were true. In that case the thresholds obtained for black and white would be merely qualitative DL's within a single series. We have, therefore, attempted to attack the problem experimentally by a more direct method. The data given below are results of a preliminary series of experiments performed in 1921, unfortunately not carried to their logical completion. Our only excuse for presenting psychophysical data based on but 10 series is the lack of any other results bearing upon the problem, the importance of the status of grey for visual theory, and the hope that some investigator may be encouraged to study the problem more intensively.

In general, our procedure consisted in obtaining (by the constant method) the stimulus thresholds for black, grey and white. The stimulus was in every case a mixture of Milton-Bradley black and white papers displayed upon a rotating color mixer. In order to have the stimuli differ by approximately equal psychological intervals, the amount of white in the mixture was varied in a geometrical series. Ten stimuli were used, namely: 6.0°, 9.4°, 14.8°, 23.2°, 36.5°, 57.3°, 90.0°, 141.2°, 220.0°, and 348.0° of white. These were presented to the *O* in haphazard order. The instructions were: "After 'ready' and 'now' signals, you will be shown a colorless disk. You are to report whether or not it shows any likeness or resemblance to, or hint of, white, reporting either 'yes' or 'no.' You are to make this report without reference to any relation of the disk to black or to grey." There were, in separate series, exactly similar instructions to judge grey without reference to black or white, and to judge black without reference to white or grey. Ten series were run under every instruction and all instructions were used (in varying orders) in every observational period. The *O*s were three graduate students (women), each of whom had had two years' experience in routine laboratory observation.

As the results are based upon but 10 observations for every stimulus, it did not appear justifiable to apply the usual mathematical procedures for the computation of thresholds. Instead we used the simple method of interpolation. But our stimuli were arranged in geometrical progression, since brightness follows Weber's Law. For this reason the values given here were obtained by geometrical (logarithmic) interpolation between the two values on each side of 50% judgments. Four thresholds were worked out; an upper limen for blackness, a lower limen for whiteness, and both upper and lower limens for greyness (all thresholds given in degrees of white).

Threshold	*Obs. A*	*Obs. B*	*Obs. G*
Grey-Lower	15	12	2[4]
Black	23	46	13
White	152	74	12
Grey-Upper	177	141	103

A consideration of these figures makes it evident that all three *O*s were not using the same criteria of judgment. Reference to the introspective reports (too lengthy to quote here)

[4]Limen obtained by extrapolation.

shows that A was judging whether or not the quality in question predominated rather than whether or not it existed at all in the mixture. As a result, her thresholds for black and for white are far apart, and grey overlaps the other two qualities but little. B, and more especially G, appear to have adhered to the instructions so far as possible. In any case, the important point is that there is no overlap of whiteness and blackness (the overlap of 1° in the case of G is not significant in only 10 series). So far as this type of procedure is concerned, grey does not seem to be intermediate between black and white any more than it is intermediate between blue and yellow. A dark grey is not whitish. It is a part of a black-grey series, not of a black-white series. In the same way, a light grey is not blackish and should be placed in a white-grey rather than in a white-black series, just as a pale blue is not yellowish and is part of a blue-grey series but not of a blue-yellow series. Preliminary results of this sort cannot, of course, be considered conclusive evidence. Further experimentation is necessary. But they may be considered as a strong indication in favor of Dimmick's hypothesis that the black-grey-white series consists of two linear series meeting at the common point of grey.

The importance of this view is obvious. If grey is not the psychological intermediate between black and white, any theory that attempts to explain it as a mixture of a constant black and a variable white excitation fails to meet the facts. Grey, it is true, may be produced by mixing physically the stimuli for black and for white (lack of light and heterogeneous light). But we may not safely argue from stimulus to sensory process. Grey may also be produced by mixing the stimuli for blue and for yellow. Yet it is not intermediate between them on a single linear series, but is the meeting point of the blue-grey and yellow-grey series which join each other at an angle of 180°. Thus we again see, as already stated in another connection, that black and white must be paired in the same way as are other complementary colors. Grey, not black, is the center from which visual experience radiates, so to speak. Titchener[5] recognizes this fact in his recent discussion of visual intensity. He points out that grey stands as the minimum of visual intensity, just where we should expect the constant visual excitation to stand.

A further question may well be raised. Why is grey so similar to black and white that it has usually been considered a part of linear series with them? A developmental answer, however valid such an answer may be, is possible. Neifeld remarks that a more primitive eye than that of man might distinguish the presence or absence of light without the sensation of black. While it is difficult to speculate upon the quality of such primordial sen-

[5]E. B. Titchener, Visual Intensity, this JOURNAL, 34, 1923, 310-311.

sory stuff, it is nevertheless logical to suppose that of all our visual sensations grey has developed the least distance from it. Black and white, more primitive than color, are most like grey; while the colors, being most developed and differentiated, are least like grey. The whole development may, perhaps, be illustrated in human vision if we remember that our eye is a dual organ. The night eye (rod vision) is more primitive and less differentiated than is the day eye (cone vision). But what experiences do we have in night vision? Merely greys of varying brightnesses. We do, of course, also see black at night. Is it due to lack of stimulation of the rods or of the cones? In view of the lack of evidence to the contrary, it is reasonable to ascribe it to the same organs as its paired experience white, that is, to the cones. The cones, then, being at a higher stage of development than the rods, mediate more differentiated types of sensation, black and white instead of grey in the first instance, followed by the later differentiation of white into colors as postulated by Ladd-Franklin. The black-white paradox, that black and white are both complementaries (mix to give residual grey) and non-complementaries (mix to give an intermediate) now disappears, since grey is not intermediate to black and white but merely the quality from which they are but partially differentiated. Black and white are to be explained in the same way as are colors.

We return, then, to our original argument. The facts of vision seem unexplainable save on some hypothesis of a constant visual process. Müller assumes that this constant process is grey, Neifeld that it is black. On *a priori* grounds there seems to be little choice unless one tries to argue from stimulus to sensation. A cortical black is just as hypothetical as is a cortical grey. But there is a great difference in explanatory value between the two hypothesis. A constant grey enables one to explain the facts of contrast, adaptation, after-image, etc., as applied to black and white by placing them upon the same basis as similar phenomena in color. It also is in accord with the evidence (as yet not conclusive) as to the nature of the black-grey-white (double) series. In all these respects the hypothesis of a constant black process meets difficulties. Whether the grey or black be considered retinal or cortical is of little importance. Morphologically, the retina is a part of the brain, and its complexity as compared with other sense organs is sufficient to explain a constant excitation not found elsewhere. But so long as the constant visual process remains in the stage of hypothesis its probable location matters but little. The outstanding question is: What type of constant visual process will best explain the facts of vision? As we have shown, a hypothetical constant grey is more adequate to the explanation of observed visual phenomena than is a postulated constant black.

AN APPROACH TO THE PSYCHOLOGY OF MOTIVES[1]

By A. J. Snow, Northwestern University

The object of this paper is iconoclastic, for it will attempt a criticism of some of the favorite psychological preconceptions of economists[2]. Among these darlings of the economic mind, two conceptions stand out. One is that there is a certain kind of action called "native" and another called "acquired." The second is that a man is a pattern of definite "instincts" or "instinctive drives," whose interactions determine his conduct. These two conceptions are closly related, for belief in the existence of definite instincts must rest on a distinction between native and acquired disposition.

Historically, these concepts were the result of the attempt to get away from the point of view that human actions are the result of abstract reason to the point of view that actions are impulsive, habitual—the result of conflicting desires, wants, and needs. As a matter of fact, however, the doctrine of instincts as an instrument explaining activity is of little value, since the value of an explanation to a scientific man lies in its predictive usefulness, and the above theory makes prediction just as futile as the rationalistic theories of the seventeenth and eighteenth centuries. To say that human activity at any given moment is the result of some native driving force does not throw any light on the situation, but on the contrary muddles it, because that ends our search and drives us to the unknown. A classification of the causes of human behavior in terms of ends or desires may give the reader with a teleological bias some moral support; but to use such classification as a scientific one must be abhorrent to many biologists.

The only way in which the doctrine of instinct would be of use would be to give it an interpretation not given it usually. We must abandon the notion of plurality of instincts, and replace that with the notion of *instinctive*[3] behavior, implying by *instinctive* action merely that all activity is a natural consequence of the interaction of the functions of the many organs that constitute the human organism and its immediate environment. Under this more sensible interpretation, instinctive activity would naturally include not only the activities of the simple reflexes and tropisms that are generally recognized as

[1]Read before the American Economic Association, Chicago, Dec. 30, 1924.

[2]For a more complete discussion see A. J. Snow, Psychology in Economic Theory, *Journ. Polit. Econ.*, August, 1924. See also *Psychology in Business Relation*, chaps. I, II, 1925.

[3]K. Dunlap, Are there any Instincts, *Journ. Abnor. Psychol.*, Dec., 1919. The Identity of Instinct and Habit, *Journ. Philos.*, Feb., 1922.

native, but also the other modifications of the inherited structures, such as habits of action as well as of thought.

This leads us to our second point, the question of the distinction between "native" and "acquired" disposition. I should say that this distinction is arbitrary, that habits are just as much the result of *native* responses of the nervous system to the environment as the original reflexes that the baby inherits. How does one tell where "native" ends and "acquired" begins? For example: at a given time the factory whistle blows, whereupon all the employees in the factory gather up their tools, put them away, and leave the workshop. In this case we have a demonstration of the tendency to react in a very definite way. No matter how complex or simple the action in this may be, it is a reaction to a particular stimulus of a particular stimulus-response pattern. The reaction pattern of course does not exist immediately preceding the blowing of the whistle. However, the factory workers did have the specific tendency. It may not have existed during the whole working day; but no one will question its existential reality when the whistle blew. It was there ready to react in a given way when the time came. Now then, what is that tendency—that stimulus-reaction pattern? Is it any more than a certain arrangement in the nervous system? Is it not an arrangement that may be described in physical, chemical, and organic terms? Is it not true that because of the existence of such an arrangement the response was made, and prediction is possible? Is this disposition to react in this definite way to a factory whistle any less native, instinctive, or is it organically different from the activities of digestion? Why, then, should the one be called "acquired" and the other "native?" Is it not more appropriate to say that all of the responses of an organism to the total stimulation are "native?" and that these are what they are at any given time because of the specific conditions of the organism and the situation to which it happens to react at that time?

We must recognize, if we study the problem of human motives without bias, that the reactions of any animal are due to the accumulated individual organic history, and that that history consequently differs for different individuals. But that is not all. The environment is an important factor: the same situation may have different meanings to different individuals and to the same organism at different times, and consequently is productive of different reactions. In fact, the idea in mind, the purpose, desire, or need that may control the individual's interpretation of any specific experience to which he reacts, or within which he acts, is in itself the result of the particular biological history of that individual. Wherefore, the specific motives or causes involved in every case of like activity of different individuals, or of the same activity at different times for

the same person, may differ, and the motives or causes involved can only be discovered accurately anew in each situation for each individual organism. While for general purposes we may classify different forms of stimulus-response patterns, we must remember that such a classification must always remain artificial, as we deal, not with any single stimulus, but always with a complexity of stimuli, and never with any single individual or independent nerve pattern, but almost always with a multiplicity of interacting and counteracting patterns, with the whole complex organism.

Therefore, the only approach to the study of human motives is not in the abstract or general but in the concrete situations. Abstract and essentially arbitrary conceptions of set instincts and equally arbitrary distinctions between "native" and "acquired" dispositions do not help us. We must make concrete studies. For example, for labor turnover we may enumerate the following causes or motives: abuse of power, wages, hours, unsanitary conditions, arbitrary advancement policy, faulty machinery or tools, heterogeneous "consciousness of kind" among team workers, personal incompatibility with immediate superiors, lack of standardized methods of process, lack of specialized training on the part of the worker, economic insecurity, physical incapacity, mental incapacity, and a mentality too great for its task. Statistics are unfortunately lacking as regards the relative importance of most of these various factors.

While the above enumeration of causes of labor turnover may be of some value, nevertheless, its usefulness is mainly for classification rather than for prediction. This sort of investigation would be better than the abstractions about instincts, but even better would be a series of concrete case-studies. In such studies we should consider concrete individuals in specific situations. For example, a study has been made of the relationship of mental alertness to labor turnover among a group of office boys in a certain factory. "In each case there has been found to exist a definite relationship between the mental alertness of employees, as determined by test scores, and labor turnover.... While admitting the danger of any study which aims to disassociate any one factor from its immediate surroundings, nevertheless, such a study may be justified on the grounds of its illuminating results and their actual use, when other factors composing the complete situation are given due weight."[4]

In this series of case-studies, as in any other, the causes valuable for predictive purposes for any specific human behavior are revealed, and only in some similar way shall we ever be able to determine the relationship that exists between human reactions and their causes.

[4]A. J. Snow, Labor Turnover and Mental Alertness Test Scores, *Journ. Applied Psychol.*, September, 1923.

FURTHER RESULTS IN "THE RANGE FOR VISUAL ATTENTION, COGNITION AND APPREHENSION" EXPERIMENT

By H. Sherman Oberly, University of Pennsylvania

Problem

In a recent number of this Journal,[1] Professor Dallenbach takes exception to the choice of the judgments selected for the calculation of the limens reported in my Study on "The Range for Visual Attention, Cognition and Apprehension."[2] He based his exception on the ground that the limens were not computed in accordance with the phi-gamma hypothesis, and states that the limens presented "give us merely the ranges of confident, correct cognitions that are immediate and ungrouped, immediate and grouped, and mediated and counted." To these 3 types of judgments we have attached the categories of 'attention,' 'cognition,' and 'apprehension.' The purpose of this paper, which was in the course of preparation before the appearance of Professor Dallenbach's note, is to present the results of the experiment in greater detail so as to include all judgments, and also to determine the extent of the correlation between the correct responses of the *O*s and the degree of assurance.[3]

Results

Tables I to VI inclusive give the results of the observed relative frequencies for each *O* in greater detail than presented in the original paper.[4] The percent. of correct responses for every stimulus value (*s*) are given under the categories of attention, cognition and apprehension. The following columns to the right contain the percent. of correct judgments for every degree of assurance reported by the *O* under every category. It will be noted that, in the present tables, the observed relative frequencies used in finding the limens for these categories are spread or separated under the degree of certainty which each *O* reported. In the last 2 columns under every category will be found the percent. of judgments to which the *O* reported an assurance of either 2 or 1 (which latter corresponds to a "do not know" judgment).[5] It will be remembered that the limens of our former Study were calculated from the combined data of the first 3 columns (assurances 5, 4 and 3) and that the results in the last 2 columns were omitted from the calculations by the setting of the problem. As 250 judgments were made for every value of the stimulus, fractionated into 5 series, the values found in these tables are given at the nearest average.

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[1] *Op. cit.*, 36, 1925, 154-156. [2] *Ibid.*, 35, 1924, 332-352.

[3] The *O* was asked to "report verbally the degree of assurance or certainty of your judgment, using this scale for your report: 5—if it is a 100 to 1 bet; 4—if it is a 5 to 1 bet; 3—if it is a 1 to 1 bet; 2—if it is a 1 to 5 bet; 1—guess or do not know."

[4] *Op. cit.*, 336-338.

[5] Results for F are not included inasmuch as he was familiar with the plan to exclude these judgments when calculating.

TABLE I Observer F

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	0.84	1.00	1.00	0.84					1.00					1.00				
3	0.64	1.00	1.00	0.64					1.00					1.00				
4	0.42	0.96	0.98	0.42					0.96					0.98		0.01		
5	0.18	0.80	0.95	0.18					0.78		0.02			0.89	0.03	0.03		
6	0.04	0.39	0.86	0.04					0.36	0.02	0.01			0.63	0.10	0.13		
7		0.08	0.69						0.05	0.02	0.01			0.26	0.18	0.25		
8		0.03	0.57						0.01	0.01	0.01			0.08	0.11	0.38		
9			0.32											0.02	0.03	0.27		
10			0.20											0.03	0.03	0.14		

TABLE II Observer M

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	1.00	1.00	1.00	1.00					1.00					1.00				
3	0.80	1.00	1.00	0.80					1.00					1.00				
4	0.49	0.96	1.00	0.49					0.96					1.00				
5	0.04	0.84	0.99	0.04					0.84					0.95	0.02	0.02		
6		0.63	0.94						0.51	0.06	0.06			0.59	0.14	0.21		0.01
7		0.16	0.77						0.04	0.07	0.05			0.05	0.18	0.54		0.06
8		0.14	0.62							0.02	0.12				0.02	0.56	0.04	0.28
9		0.02	0.16								0.02					0.10	0.06	0.68
10			0.01														0.01	0.83

TABLE III Observer S

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	1.00	1.00	1.00	1.00					1.00					1.00				
3	0.95	1.00	1.00	0.95					1.00					1.00				
4	0.59	0.96	1.00	0.57	0.01	0.01			0.92	0.03	0.01			0.95	0.03	0.02		
5	0.09	0.75	0.98	0.08	0.01				0.44	0.25	0.06			0.56	0.34	0.08		
6	0.02	0.76	0.96	0.02					0.26	0.32	0.15	0.03		0.32	0.40	0.22		
7	0.01	0.57	0.83				0.01		0.02	0.18	0.25	0.11		0.03	0.28	0.37	0.03	0.04
8		0.65	0.83							0.11	0.34	0.20			0.14	0.45	0.04	0.05
9		0.33	0.42							0.01	0.11	0.21			0.01	0.15	0.05	0.40
10		0.14	0.21									0.14				0.02	0.05	0.65

TABLE IV Observer Mc

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	0.73	1.00	1.00	0.73					1.00					1.00				
3	0.78	0.96	1.00	0.78					0.96					0.99				
4	0.32	0.99	1.00	0.32					0.98		0.01			0.99		0.01		
5	0.13	0.87	0.97	0.13					0.85	0.02	0.00			0.94	0.03	0.00		
6		0.74	0.83						0.45	0.23	0.05	0.01		0.52	0.24	0.06		
7		0.57	0.67						0.11	0.30	0.14	0.02		0.14	0.35	0.16		0.04
8		0.58	0.67						0.08	0.31	0.16	0.03		0.08	0.35	0.20		0.06
9		0.27	0.31							0.05	0.15	0.07			0.06	0.17	0.01	0.34
10		0.23	0.25							0.03	0.09	0.11			0.03	0.10	0.01	0.51
11		0.10	0.11							0.01	0.01	0.08			0.01	0.01	0.01	0.71
12		0.01	0.02									0.01					0.01	0.88

TABLE V Observer SC

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	1.00	1.00	1.00	1.00					1.00					1.00				
3	0.99	1.00	1.00	0.99					1.00					1.00				
4	0.96	1.00	1.00	0.96					1.00					1.00				
5	0.54	0.98	1.00	0.54					0.98					0.98	0.01	0.01		0.01
6	0.12	0.95	0.98	0.12					0.83	0.06	0.05	0.01		0.85	0.07	0.05		0.00
7		0.81	0.84						0.48	0.17	0.15	0.01		0.51	0.18	0.15		0.01
8		0.76	0.81						0.18	0.30	0.27	0.01		0.19	0.32	0.28	0.01	0.01
9		0.54	0.56						0.04	0.18	0.29	0.03		0.04	0.18	0.29	0.02	0.06
10		0.38	0.38							0.04	0.30	0.04			0.04	0.30	0.00	0.10
11		0.41	0.42							0.01	0.26	0.14			0.01	0.26	0.01	0.10
12		0.30	0.31								0.17	0.13				0.17	0.00	0.24

TABLE VI Observer JC

% Correct Response				Attention					Cognition					Apprehension				
S	Att.	Cog.	App.	5	4	3	2	1	5	4	3	2	1	5	4	3	2	1
2	0.73	1.00	1.00	0.73					1.00					1.00				
3	0.68	0.99	1.00	0.68					0.99					1.00				
4	0.44	0.64	0.98	0.44					0.65					0.98				
5	0.05	0.38	0.98	0.05					0.38					0.96	0.01	0.01		0.01
6		0.33	0.92						0.31	0.01		0.01		0.82	0.05	0.03	0.01	0.01
7		0.11	0.91						0.05	0.04	0.01	0.01		0.47	0.29	0.12	0.02	0.03
8		0.05	0.73						0.01	0.02	0.01			0.05	0.09	0.35	0.23	0.09
9			0.35											0.01	0.01	0.06	0.27	0.40
10			0.23													0.03	0.20	0.60
11			0.06														0.06	0.77
12			0.04														0.04	0.91

In Table VII will be found a record of the observed relative frequencies of incorrect judgments for all of the *O*s. A column is given to every *O* and the results for every value of stimulus dots is to be found in a single row. This table is given for the sake of completeness in presenting our results. It will be observed that the percent. of wrong judgments varies considerably from *O* to *O*. For Mc and JC wrong judgments were given for even the smaller stimulus values. For M, S and SC no wrong judgments are given on smaller values of the stimulus, although for SC higher values are considerably greater than for the other *O*s.

From Tables I to VI it would be possible to calculate the limens for the categories of judgment set by our problem in the manner which Professor Dallenbach desires. We believe, however, that we were not only justified in excluding the lower degrees of assurance from our calculations, but still believe that it was the desirable thing to do. In former psychophysical work little attention has been taken of the degree of assurance of the *O*, which means no attention has been taken of the variation of conservatism from *O* to *O*. Urban[6] made such an attempt for lifted weights

TABLE VII

S	F	M	S	Mc	SC	JC
2	0.00	0.00	0.00	0.00	0.00	0.00
3	0.00	0.00	0.00	0.00	0.00	0.01
4	0.01	0.00	0.00	0.00	0.00	0.01
5	0.01	0.01	0.02	0.03	0.00	0.01
6	0.01	0.05	0.04	0.17	0.02	0.07
7	0.02	0.17	0.13	0.29	0.14	0.07
8	0.05	0.11	0.12	0.27	0.18	0.18
9	0.06	0.16	0.18	0.35	0.38	0.25
10	0.05	0.06	0.14	0.24	0.52	0.17
11				0.18	0.48	0.16
12				0.10	0.45	0.05

but abandoned it as leading to a too complicated protocol. It will be remembered however that Urban was dealing with a difference threshold which has as its basis a much more complicated procedure than that which we were requiring for our *O*s. The protocols of the *O*s in the present experiment indicated in every case that the requirements of our instructions were not too complicated to be fulfilled with reasonable facility. It is perfectly obvious that the degree of conservatism between different *O*s varies and that different judgments are made on varying degrees of assurance. Your conservative *O* will report only those judgments which have a relatively high degree of assurance. The unconservative will report on the slightest cue. Little or nothing has been done in psychophysics of the past to control or standardize this factor. The introduction of "guess" judgments, although it is an attempt to control this factor, can of course, be of little use until the "guess" judgment itself is more exactly defined. In other words, that which shall be included in a guess judgment is still determined by the degree of conservatism of the individual subject.

Hence our attempt, crude though it may be, to define 5 degrees of assurance on the basis of a scale of betting odds. After a good deal of consideration we failed to think of any scheme by means of which we could express this assurance in psychological terms, and we therefore employed this widely familiar but extra-psychological definition of our degree of assurance.

It would of course have been possible and would have obviously been the complete procedure, to calculate the constants of the psychometric

[6] F. M. Urban, *The Application of Statistical Methods to the Problems of Psychophysics*. 1908.

functions for each of our degrees of assurance. The labor involved in this would have been very great indeed and we still fail to see what advantage can be gained from such a series of calculations. The data are herewith presented for anyone who cares to make them.

It will be remembered that our problem was one in which we wished to *compare* thresholds for three categories of judgment from the same subject and under the same conditions. Therefore, so long as the conditions of calculation were the same, these comparative results have validity. We perfectly arbitrarily excluded all judgments made on the basis of assurance less than of an even money bet inasmuch as it seemed to us that any judgment with a lower degree of assurance was in the nature of a "guess."[7] Certainly these conditions were clearly stated in our original paper. We still believe that this procedure of standardizing our degree of assurance is one which should be taken into account in future psychophysical work inasmuch as our results seem to clearly indicate the necessity for controlling such a factor.

A study of Tables I to VI indicates that for all *O*s and for everyone of the 3 categories of judgment, assurance 5 is given with a relatively high degree of frequency for the lower stimulus values, while as the number of stimulus dots increased the relative frequency of 5 judgments gradually

TABLE VIII

	5			4	
O	Att.	Cog.	App.	Att.	Cog.	App.
F	+1.00	+1.00	+0.98		+0.50	+0.26
M	+1.00	+1.00	+1.00		+0.50	−0.05
S	+1.00	+1.00	+0.96	+0.50	+0.36	+0.37
Mc	+1.00	+0.99	+0.96		+0.35	+0.46
SC	+1.00	+1.00	+0.96		+0.49	+0.09
JC	+1.00	+1.00	+0.99		−0.50	−0.07

	3			2	
O	Att.	Cog.	App.	Att.	Cog.	App.
F		+0.80	−0.75			
M		+0.40	−0.40			+0.40
S		−0.48	−0.10		−0.50	−0.70
Mc		−0.31	−0.36		−0.28	+0.50
SC		−0.53	−0.62		−0.79	+0.65
JC		+0.50	−0.35		+0.50	−0.32

becomes smaller and smaller and the frequencies of first the 4 and then the 3 judgments gradually increase. A consideration of the results for our category of apprehension shows this most clearly. It was thought therefore worthwhile to calculate the coefficient of correlation between the different values of the stimulus and the degrees of assurance for everyone of the 3 categories of judgments as originally published. The Pearson rank order method was used in these calculations.

The results will be found in Table VIII. In the first column will be found the initials of the different *O*s. The table is divided into 4 groups of 3 columns each. In each group will be found the results for the 4 degrees of assurance and in each column the coefficients for each of our 3 categories of judgment. It will be observed that the coefficients of correlation for as-

[7] All of the *O*s except F worked without knowledge of the procedure and it will be observed that the results of F for assurances 1 and 2 are not included.

surance 5 are all so high as to be of greatest mathematical significance,—12 of the 18 being a perfect positive correlation. 5 of the 6 results that are not of perfect positive correlation are found in the column of assurance 5 for apprehension, which it will be remembered will be found as our most complex form of judgment. Very few of the other coefficients found in the table are of great mathematical significance. Indeed, they were of such little mathematical significance it seemed hardly worthwhile to work out their probable errors. It will also be observed that in the case of only one *O*, (S), does any coefficient appear except for assurance 5 (a coefficient of +0.50 appears for S for assurance 4). This means that for all *O*s except S, all attention judgments were given with assurance 5. A study of Table III will show what a small percent. of judgments were given by S with assurance other than 5.

This may mean two things. (1) It may mean that we should have included only judgments with assurance of 5 in our calculations. In other words, we have been perhaps too lenient in admitting judgments given as low as an even money bet assurance into our calculations. (2) The exceedingly high positive correlations for attention, cognition and apprehension with assurance 5 (which indicates almost positive certainty) show that all 3 of these systematic categories, as we have defined them, are proper bases for experimentation under the problem as we have set it.

Conclusions

(1) Further treatment of results indicate that attention, cognition and apprehension, as defined, are proper systematic categories for psychological experimentation.

(2) Our attempt to standardize the degree of assurance of the judgments of the different *O*s seems to be valid, although the present results show that in our former study it might have been better to include only those judgments which were given with the highest degree of assurance and to have still further eliminated from the calculations some of the judgments given with a lower degree of assurance which were included.

CHANGES IN AUDITORY ACUITY DURING THE PERFORMANCE OF CERTAIN MENTAL TASKS

By Lee Edward Travis, State University of Iowa

Three years ago the writer began a series of experiments[1] upon changes of sensitivity during certain artificially induced mental states. It was found that some normal individuals and dementia praecox patients experienced reduced cutaneous and auditory acuity during crystal-gazing, reverie and day-dreaming, while other normal persons and psychoneurotics experienced heightened cutaneous and auditory acuity during these states. The mental processes induced seem to be those of unrestrained association. The *Os* attend only to images and ideas that arise involuntarily in consciousness. Their attention shifts from one image or idea to another without any internal effort being made to direct the trend. If a variation in the liminal value can be brought about by unrestrained association it was thought worth while to ascertain if a variation could be produced by some other mental process, such as reasoning, memorizing, or imagining.

Hamlin,[2] in 1896, performed some experiments for the purpose of measuring the effect of attention upon sensible intensity. She found that addition, as the distracting task, could not be used effectively together with judging small differences of intensity, brightness or pressure, because "the subjects usually found that it acted as a spur rather than as a check to the attention. Their attention was wholly absorbed in the addition for a part of the interval, but as a rule the distraction was not continuous, and there came free instants in which the stimulus flashed into consciousness with great clearness and distinctness."[3] This seemed to indicate that sensitiveness for judging small differences was increased rather than decreased by the distracting task.

Peters,[4] in 1906, approached the problem from the measurement of attention. He presented his *Os* with stimuli under three conditions: (1) attention concentrated upon the expected impression; (2) attention concentrated upon a different activity,—as reading, arithmetic or dynamometer tensions; and (3) attention directed to whatever stimulus (light, noise or pressure) that might be given, without expecting any particular one. Conditions (1) and (2) are comparable to those of this study. Peters found a uniform rise of the limen above the normal value under the second condition (attention concentrated upon a different activity). This furnishes experimental evidence for the assumption that concentration with *Einstellung* will give a lower liminal value than concentration without *Einstellung*. Thus we see that these two experimenters arrived at quite opposite conclusions. Since the work of Peters nothing is to be found bearing directly upon the effect of various mental processes on liminal values.

The results of the present paper corroborate those obtained by Hamlin.

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[1] L. E. Travis, Studies in dissociation, *Jour. of Exp. Psych.*, 5, 1922, 338-346; Suggestibility and negativism as measured by auditory threshold during reverie, *Jour. of Abnorm. Psych. and Soc. Psych.*, 19, 1924, 350-368; Test for distinguishing between schizophrenoses and psychoneuroses, *ibid.*, 283-298.

[2] A. J. Hamlin, this Journal, 8, 1896, 1-66. [3] *Op. cit.*, 49.

[4] W. Peters, *Arch. f. d. ges. Psych.*, 8, 1906, 403.

Method

The apparatus for determining the auditory *RL* has been described in an earlier Study.[1] Instead of the crystal-gazing situation of the other experiment, various written materials were presented to the *Os*. This material consisted of 17 mathematical problems of sufficient difficulty to be interesting and absorbing yet easy enough to be calculated without the use of paper and pencil; Pearson's discussion of 'Matter' in his "Grammar of Science;" a poem, "The Moral Warfare," by Whittier; and a love-story, "The Victory of Connie Lee," by Aldrich. The materials were presented to the *Os* in the order named.

The *O*'s so-called 'normal threshold' was first of all ascertained. This was found with attention upon the incoming tones. Then the mathematical problems were presented and *O* was directed to solve them, leaving his finger, however, on the response key in order to respond whenever he heard any tones during the solution of the problems. After the *RL* had thus been obtained the *RL* under normal conditions was again found. Then the *RL* during the reading of the passage was ascertained, and then again under normal conditions; and so on for the other tasks, giving 9 measurements of the liminal value for every *O*. The comparisons pertinent in this Study are those made between the *RL*s obtained during the exercise of any mental function and the *RL*s obtained before and after that exercise.

Table I
Readings on Individuals

	*Thresholds**											
	Solving Problem			Reading Pearson			Memorizing Poetry			Reading Love-story		
O	Before	During	After	Before	During	After	Before	During	After	Before	During	After
A	12	10	12	24	20	26	20	18	20	26	14	16
B	25	16	30	10	6	10	25	9	10	19	10	19
C	6	1	5	4	1	3	5	2	4	3	3	7
D	14	4	6	6	5	9	6	5	8	6	4	6
E	5	3	5	5	3	4	5	4	4	4	2	5
F	28	15	28	9	10	15	15	11	20	20	7	13
G	12	1	12	5	15	5	12	11	18	5	4	10
H	11	10	13	8	23	6	14	20	14	14	26	8
I	27	23	27	—	—	—	4	4	4	14	10	14

*These thresholds are expressed in terms of ohms resistance.

Results

Nine *Os* served in this Study; 8 were graduate students in psychology, and 1 was a patient of the psychopathic hospital. The latter, a case of conversion hysteria, a school teacher of ability and success, was, however, essentially adjusted in all spheres.

Table I shows that the majority of the *Os* experienced a decrease of the *RL* during the performance of all four of the mental tasks. Only 1 *O* showed an increase in the *RL* in three of the mental operations. No *O* failed to show a decrease of the *RL* during the performance of at least one of the mental tasks. Let us take the four mental functions separately.

[1] *Jour. of Abnorm. Psych. and Soc. Psych.*, 10, 1924, 350-368. The stimulus was a clear tone of constant pitch. The intensity of the tone was varied by means of a series of non-inductive resistances. The threshold of the "normal period" was ascertained with the attention concentrated upon the incoming tones. During the solution of the problems, the memorization of the poem and the reading of the two forms of printed matter, the attention was directed upon the different activities. *O* was told, however, that he would be given tones, and, if he should hear any, to respond as he had done previously.

During the solution of the problems every *O* reacted with a lowered *RL*; during the reading of the passage from Pearson, 5 had lowered *RL*s, 1 had a *RL* between that of the pre- and that of the post-reading periods, and 2 had raised *RL*s, 1 *O* was not tested in regard to this operation; during the memorization of the poetry, 6 had lowered *RL*s, 1 a raised, and 2 experienced no change; and finally, during the reading of the love-story, 7 experienced a lowering, 1 a rise, and 1 no alteration of the *RL*. These facts are more clearly shown in Table II.

TABLE II
Summary of Table I

	During Solving Problems	*During Reading Pearson*	*During Memorizing Poetry*	*During Reading Love-story*
No. of *O*s with lowered *RL*	9	5	6	7
No. of *O*s with raised *RL*	0	2	1	1
No. of *O*s with no change in *RL*	0	0	2	1
No. of *O*s with *RL* between that of the previous and subsequent periods	0	1	0	0

DISCUSSION

One of the main conclusions of a previous Study by the author was the identification of suggestibility with a lowered *RL* and of negativism with a raised *RL*. As has been stated above, the characteristic mental state of crystal-gazing is that of unrestrained association, and from the results of the Study it seemed that some individuals were capable of entering into this state while others were not. The hysteric and the suggestible normal individual seem to be able to lose themselves in reverie and approach a state of auto-hypnosis while the dementia praecox and the negativistic normal individual resist such change. Thus the ability to become mildly dissociated under crystal-gazing conditions is prerequisite for a lowered threshold or heightened sensory acuity. The lack of this ability would give no change in the threshold while active or passive resistance to any such procedure as crystal-gazing or unrestrained association would give a rise in the liminal value. The crystal-gazing situation then becomes the hinge on which swings the differentiating power of the test. It is not to be thought that the present Study can be used for any differential indicator, for some of the individuals who experienced a rise in the crystal-gazing threshold had a lowering of the limen during some or all of the mental operations used in the present experiment. This can be explained on the basis that there is a very marked difference, if not an opposition, between free association and the attitude of facing a task such as solving problems or memorizing poetry. Individuals who would be opposed to the former and hence fail to lose themselves in the crystal-gazing situation may be able to become absorbed in impersonal tasks or mental operations that would not involve catching them "off-guard" in regard to some of their personal fancies.

Our interpretation of the data of this Study would be then that a lowered *RL* or heightened sensory acuity in regard to an impersonal mental operation is indicative of complete absorption in that mental task or operation and that a heightened *RL* during the performance of a certain mental function is indicative of inability to become completely lost in the operation, or of an inhibition to the execution of the task. This is borne out by

introspective data gathered from the *Os*. Every *O* found the problems interesting and enjoyed doing them, and it is to be noted that they experienced a lowering of the *RL* during their solution. *F*, *G* and *H* thoroughly disliked the dry and involved discussion in the passage from Pearson, and the table indicates that their *RL*s rose during the performance of this task. *H* also found the memorization of the poem too much like work and the love-story not at all interesting, and consequently he failed to get into the tasks at all.

This conclusion raises certain questions in regard to attention. It seems to indicate that a fainter tone can be heard if attention is not focused directly upon the tone but upon something else. The teachings of current psychology[6] hold that attention upon a stimulus increases the intensity of the stimulus, while distraction decreases the intensity. For example it is taught that one can hear with attention, a faint sound that one cannot hear if he does not attend. Now the difficulty seems to be in the idea that one has to attend to the faint sound in order to hear it. The results of this Study indicate that the function of attention, rather than the direct object of attention, is requisite for heightened acuity and to attend to something else would be more conducive toward hearing the faint sound than to attend directly to the sound itself. This apparent contradiction might be explained by the fact that if heightened sensory acuity during the performance of a mental task indicates complete absorption in that task, then just attending to tones is not sufficient to bring about a state of complete absorption and hence not capable of producing maximal sensitiveness of the organism. This would seem to be the case as there is not sufficient challenge in a simple tone to command complete absorption of attention. Thus it appears in the last analysis that the *degree* of attention determines the liminal value of a stimulus regardless of whether the stimulus is the object of primary attention or some other object that may come under the direction of attention.

Experimentalists[7] interested in attention have published much in regard to its effect upon the apparent intensity of the stimulus, but their works have added nothing new to the relevant theories of attentional intensification in the ordinary sense. Most of these theories offer a physiological explanation or nervous correlation which generally means a facilitating or reinforcing process.

The present Study does not offer a new theory of attentional intensification or necessarily a contradiction of old ones but it seems to have opened up a new way of looking at the problem. Instead of seeing a facilitating or reinforcing process in connection with only the sensory field attended to, may not one view it as rather a reciprocal innervation process spreading to all sensory fields and thereby causing generalized heightened acuity.

More extensive research along many lines of approach is necessary to determine further the value of a new point of view of attention.

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[6] E. B. Titchener, *A Text Book of Psychology*, 1909, 280. W. B. Pillsbury, *Attention*, 1908, Chap. I.

[7] In a recent work by S. N. Newhall. Effects of attention on the intensity of cutaneous pressure and on visual brightness, *Arch. Psych.* 9, 1923, 5-75, we read: "we may apparently conclude in general, that attention increases the intensity of moderate and liminal areal pressures, and the degree of moderate visual brightness when near the difference-limen. The results in both sense fields seem to indicate that on the one hand attention increases the number of stimuli which are superthreshold, and on the other hand, that attention increases the intensity of pressure and the degrees of brightness of the respective stimuli when they *are* superthreshold."

BOOK REVIEWS

Der Strukturbegriff in der Psychologie. By FELIX KRUEGER. Jena, Gustav Fischer, 1924, pp. 26.

The concept of structure is reviewed in its historical, philosophical and psychological settings in this little *Sonderabdruck* from the reports of the eighth congress for experimental psychology held at Leipzig (1923). Interwoven with all of this material is a criticism of the concept of structure as it has been used by the members of the school of *Gestalt*. While asserting that problems of individuality and personality demand the concept of psychic structures and that totalities, wholes, must be studied for their own sake descriptively, Krueger attacks such attempts as have been made to bring order out of a welter of opinions. We thus find both praise and blame which makes no distinctions: Wundt is given credit for having stressed *Ganzheitsmomente* along with Wertheimer, yet no distinction is made between the absolutely different points of view from which these two men approach the problem of configuration. The author insists that *Gestalten* may be single atoms while the whole still remains to be treated. But precisely what the totality is and how one would go to work in the laboratory to investigate it, remain *dahingestellt!* Krueger's scythe cuts both ways; the path he tries to follow is too broad and leads him in circles.

A structured *Seelenwesen* is the real necessary presupposition for all psychic phenomena. A more enduring reality must be postulated for psychic events. Dispositions are enduring. By means of analysis and synthesis we can study part structures. Then the old psychology comes in for its share of criticism in following the false idols of physics and speaking in terms of fusion of elements and the like. Psychology must have a system of laws but talking about the total universe certainly will not give it to us. No definite constructive picture emerges from Krueger's discussion: it is a bundle of notions in the jargon of philosophy, psychology, logic, and theory of value and out of this no scientific methodology emerges.

Some of Krueger's criticisms are to the point if one takes the trouble to disentangle them from an almost hopeless context. Several of them are as follows. Not every totality is a configuration. Temporal configurations offer many difficulties (See the reviewer's article on "The Psychology of *Gestalt*" pp. 25-62 supra). Wertheimer and his followers have not paid enough attention to the feelings as integral and important elements in the total psychic life of the individual. The word *Gestalt* should be limited, as Müller has pointed out in his book *Komplextheorie und Gestalttheorie*. Formed character and total character must be distinguished.

Just as his criticisms of *Gestalt* and other concepts are meagre, so are his comments on phenomenology which is playing such a prominent part in German psychology. Krueger points out that the assumptions and assertions of phenomenology, no matter what they may mean for logic, require thorough psychological investigation. The presence of an act in all experience does not seem to him a proved fact, indeed, it is contradicted by the unreflective behavior of animals and children. The further criticism of normative and metaphysical abstraction in phenomenology do not seem to the reviewer to concern the phenomenology which one finds in present-day psychology. However, Krueger has hinted at several things which make us await with interest the promised book to appear on the concept of structure in psychology.

University of Illinois HARRY HELSON

Old and New Viewpoints in Psychology. By KNIGHT DUNLAP. St. Louis, C. V. Mosby Co., 1925, pp. 163.

The lectures and papers contained in this little book were in part designed for popular audiences, but because of certain systematic and scientific views they invite attention from the psychologist. The chapter on mental measurement, the first lecture, presents a sound view of the aims and methods of individual psychology. The author expresses the fact too seldom recognised that mental tests measure intelligence, defined as the ability to acquire knowledge, indirectly by measuring acquired knowledge. Very apt also is the warning that the tests are merely tools and are valuable only in proportion to the insight and psychological training of the tester, and the reminder that mental measurement as an applied science can proceed no faster than purely scientific research will permit.

The second lecture attempts to show that certain schools of psychology have been nothing more than eddies in the main stream of 'plain psychology.' Although the author is quite able to define these by-movements, such as behaviourism, Freudism, and instinct-psychology, aside from a doubtful characterisation of the 'reactionary structuralists' —whose method he confuses with the method formerly known as introspection—he does not state specifically what the methods or problems of 'plain psychology' are. The point that specialists in other fields regard the domain of psychology as a proper field for speculation will be appreciated by all psychologists.

The remaining chapters, especially those on spiritism and on character analysis, invite the layman to take a scientific attitude toward the problems in these fields.

On the whole, one comes away from the book with a feeling that the title is a misnomer. Only in the second chapter does the author touch upon fundamental points of view, old or new; the remainder of the book is given over to the less fundamental by-products of the science.

Cornell University J. P. GUILFORD

Pleasure and Behavior. By F. L. WELLS. New York, D. Appleton and Co., 1924, pp. xvi, 267.

The purpose of this volume, we are told in the introduction written by Joseph Jastrow, editor of the *Science of Mind Series* in which this book appears, "is primarily to set forth the foundations of the affective life, so that a comprehension of the natural setting of pleasure in its relation to behavior may lead to a truer insight into the control and the ordering of desire in the artificial setting of today" (xv). Since the aim of the volume is practical "the author assumes responsibility for its scientific character" (xi). To the layman, therefore, the book may appeal as an interesting set of observations upon behavior, morals, marriage, and the like, from the standpoint of a psychologist; to the trained psychologist it seems to be a concatenation of common-sense observation presented in the form of hasty generalization supported by doubtful scientific evidence and a hopeless confusion of fundamental psychological concepts which should be handled clearly and cautiously in popular as well as in scientific discussions.

The terms brain, mind, behavior, memory, attention, and even psychology itself, are used indiscriminately and with no precise or traceable meanings. If we take the definition of psychology offered by the author seriously then it is hard to understand what is meant by a great deal of what follows. Thus we are told that the subject-matter of psychology is the "behavior of the organism as a whole," which is certainly not objectionable if one cares to define it in that way, but when the author proceeds to speak of "the will to activate a striped muscle group" (11), of bodily and mental pleasures, of memory as an after-image, and the control of

attention, it is hard to know precisely what he means. After the foregoing definition of psychology the following statement seems to violate the canon of consistency: "The pleasures of imagination and hope can be described in mental terms alone" (30). In fact it is asserted that there may be a "blending" of bodily and mental factors in pleasures. Some statements taken merely on the popular level are incomprehensible to the reviewer: "The brain keeps memories intact and serviceable." "The psychological difference between smooth and striped muscle lies in that the latter contracts and relaxes promptly on receipt and cessation of its impulse; while smooth muscle is very much slower to act" (10). A reflex "is aroused by instinct and stimulated by trial and error" (14). So much for the psychological medley.

Fields other than psychology come in for consideration in this book since the quest of pleasure may lead one anywhere. The three great sources of pleasures are friends, wealth, and love, hence social, economic, moral, and legal matters must be discussed as they bear on the satisfactions of the individual. Thus a consideration of sexual pleasures leads to the problems of the double standard, divorce and marriage. We find that "inward traits of personality are rather unfavorable to unions as close as marriage" (137). Psychology and sociology join hands in a consideration of intelligence and social organization, language and communication. The relative importance of instinct and intelligence for social organization seems never to be clearly settled for, while the author seems to adhere to McDougall's view that the instincts are the prime movers of society, he asserts that "intelligence" is more important than instinct after all. Again we are told that intelligence alone is impotent, instinctive drives are necessary. When we come to the complex types of behavior with which law, ethics, and religion must deal the pleasure principle is supplemented by "loyalty to an in-group" and the statement is made that "goodness and happiness may be brought into amicable relations" (162) with the implication that something more than the hedonistic principle is necessary to bring them together. The two fundamental postulates seem to be that the satisfaction of fundamental drives gives pleasure, thwarting them gives pain. Religion, comradeship, wealth and love may serve important compensatory functions in life for fundamental drives which cannot be directly satisfied (sublimation?). But the pleasure principle still needs a prop for "the acclaim of history is for those who gave to life; the pleasure value to the giver is a secondary consideration." The obvious implication is that while pleasure may influence behavior there are other causal factors at work in human motivation.

One wonders what scientific evidence there is for several generalizations of the following sort: "Women, in general, find more pleasure in the courtship of men who have passed through a baptism of love." "Save as love-objects men and women do not understand one another so readily as they do those of their own kind." "History indicates that the dominant peoples of the world have been those that responded to Christianity." "It takes a much more developed mind to worship something seen with the eye of faith alone."

The book will appeal to many people interested in a superficial way in a variety of problems gathered together under attractive headings. The style is simple and attractive. The comments and observations betoken a wide range of reading. The book is suggestive of what an applied psychology might be. It cannot be taken seriously by the reader seeking psychological principles or their application to definite problems. It is hard to say for whom the book is suited in view of its slight scientific value and the wrong impression it may give the lay reader of what psychology really is.

University of Illinois HARRY HELSON

The Problem Child in School. By MARY B. SAYLES. Joint Committee on Methods of Preventing Delinquency, 1925, pp. 287.

Three Problem Children. Joint Committee on Methods of Preventing Delinquency, 1925, pp. 146.

The Visiting Teacher in Rochester, By MABEL BROWN ELLIS. Joint Committee on Methods of Preventing Delinquency, 1925, pp. 205.

Data collected during the last fifteen years have made the educational profession acutely conscious of the child who fails to make normal progress in school. Whether retardation be due to low mental ability, or whether it be caused by poor teaching, a formal curriculum, deficient eyesight, lack of proper encouragement in the home or any one or more of many other factors, it is obvious that society is vitally concerned. Not only do the repeaters increase the cost of the schools but they tend to drop out of school early and often become problems in delinquency for the civil authorities to deal with. In the three volumes named above the Joint Committee on Methods of Preventing Delinquency has presented some aspects of its work. The first volume analyzes some 25 cases of children classified as problems because of parental attitudes, feelings of inferiority, dishonesty, or sexual abnormality. In each case the situation in which the child was found is fully described and the methods employed in attempting to meet the situation are given. In the second volume three cases are described in considerable detail while the results of the physical and psychological examinations are given in the appendix. In the third volume the work of the visiting teacher as it is carried on in the city of Rochester, N. Y., is described. Information concerning the development of the visiting teacher in that city is presented and the organization explained in some detail. About half the book is devoted to "What she does and how" in which the visiting teacher is followed from hour to hour and her methods of work described. In the year 1923-24 this department of the Rochester schools dealt with 3,894 cases. All three volumes are written in untechnical language, and, since the case method is generally employed, the stories given are gripping. Parents, teachers, and administrators will find a perusal of these volumes much worth while. Further activities of this Committee should be followed with interest by all socially-minded persons.

Cornell University JULIAN E. BUTTERWORTH

Instinct; A Study in Social Psychology. By L. L. BERNARD. New York, Henry Holt and Company, 1924, pp. viii, 534.

This book is an argument against certain conceptions of instinct; it is an avowed attempt to show that the psycho-social environment is of far greater importance in the making of the human individual than is heredity. The argument begins with the assumption that, since instinct is generally defined as inherited behavior, it must be inherited according to Mendelian units. Such a unit cannot be a sophisticated abstraction or even a "tendency" to act in a certain direction, but must rather be some definite nervous structure of which the behavior is a result. The author defines instinct as "a specific response to a specific stimulus or set of stimuli."

After discussing the nature of instinct and its organic basis, the author presents a survey of the literature, scientific and non-scientific, which shows all possible uses of the term. The list, secured from 323 authors, includes 847 classes of general instincts and 5,684 specific responses grouped into 1,594 classes. Testing these instincts in the light of his own definition, he finds that not one is a specific response to a specific stimulus. By analysis there may be found in every one, *e.g.*, the so-called instinct of criminality, a large number of more or less definite responses which may also be found in many other so-called instinctive acts. The question now arises, what is

the organising agent which brings these interchangeable units into an integrated act which has been characterised as an instinct? The author rejects the assumption of McDougall that emotion is the integrating agent, the core, or characteristic part of the instinct,—as for example, the tender emotion is the core of the maternal instinct,—chiefly because there is no original structure for this emotion which can be inherited as a Mendelian unit. He maintains that there is only one other possible organising agent, and that is the environment. The inborn basis of behavior is merely a few discrete, simple responses to stimuli upon which environment builds an immense superstructure by supplying the proper stimuli or combinations of stimuli. What McDougall and others have called instincts are really habit-complexes which continue to grow under the influence of environment until "the child who has reached a rational age is reacting in nine-tenths or ninety-nine one-hundredths of his character directly to environment and only in the slight residual fraction of his nature directly to instinct." Society has only to manipulate the child's environment in order to control his education.

This book is undoubtedly the most noteworthy systematic contribution to the subject since McDougall's *Introduction to Social Psychology* and one which will be heartily welcomed by sociologists, and perhaps by educators. The survey of the literature is very complete and the author's conclusions form the basis of his attack. The book also opens up new avenues of investigation. Are there Mendelian units of behavior? If so, what are they, and how complex may they be? What is the organising effect of environment or what is the relation of environment to integrated behavior?

The argument, however, may be partially motivated by the desire to find controllable factors in the social environment of the child. The *reductio ad absurdum* of the instinct of criminality is made easy by choosing an instinct regarded as absurd without reduction by many scientists. The problem of integration of the unit responses into functional complexes may be solved in more than the two ways discussed, as the *Gestalt* psychologists have suggested. To substitute the environment for emotion as the organising principle merely shifts the problem of integration from mind to the stimuli which act upon it.

Cornell University J. P. GUILFORD

Hypnotism and Suggestion. By LOUIS SATOW. Translated by Bernard Miall. Dodd, Mead and Co., 1924, pp. 283.

This book is written for the lay reader, and is more an historical treatise on the origins of our present forms of suggestion—hypnosis, religion, war, panic, "booms," rabid nationalisms, etc.—than an effort at a psychological analysis of the subject. Though the book is provided with a glossary adapted from Paul's translation of Baudouin's "Studies in Psychonalysis," no reports of original researches on the nature of the phenomena are given.

The book deals quite adequately and succinctly with the history of hypnotism; sketches briefly the origin of religion; discusses the theories of the relation of mind and body; and, above all, is frankly a philippic against religion, nationalism and militarism. The chapters on the phenomena of hypnotism and therapeutic suggestion follow the usual lines (*e.g.*, of Moll, Wetterstrand, Grasset), except that the details here are not so numerous, nor are both sides of doubtful questions given. For example; it is taken for granted that the perceptual powers can be greatly augmented in hypnotism (p. 120), and that posthypnotic amnesia is the differentiating characteristic of the deepest, the somnambulistic, stage of hypnotism (p. 76). The Reviewer's researches in hypnosis (*cf.* this JOURNAL, 36, 1925, 214-232) would lead him to deny both of these assumptions. The chapter on mass psychology is sanely written.

Satow traces hypnotism and all other forms of suggestibility to native credulity resting on an instinct of imitation. The remedy for the present evil state of mass-mindedness which is seen in all religions (no distinctions are made), in our narrow nationalisms, in race hatreds, and in militarism, is knowledge. The education of the future must raise the individual above the level of mass suggestibility. Yet, strangely enough, by continued propagation of the right ideas these ideas will filter down to the subconscious minds of the masses; so that, after all, we shall be taking advantage of the blind force—suggestibility—that now enslaves the world. The optimism shown in the last few pages, after the black picture painted in the rest of the book, gives it almost a Shellyian aspect.

Louisiana State University PAUL CAMPBELL YOUNG

Lazarus. By HENRI BÉRAUD, translated by Eric Sutton. New York The Macmillan Co., 1925, pp. 187.

This book will be of special interest to students of the abnormal phases of psychology, for the story is about a case of double personality. The author draws a vivid and dramatic picture of a man who has forgotten his past and who is now haunted by his former self. The story opens with the awakening of Jean Mourin, a musician, a man with all the refinements and the temperament of that profession. He is in a hospital which he recognizes though he knows perfectly well that he has never before seen it. Because he feels no discomfort, no illness nor injury, he calls out; and when the nurse comes he demands to know, to her utter amazement, where he is and why. He is overwhelmed to learn that sixteen years previously he had been seriously injured in a motor accident in which his wife and all the other members of the party had been instantly killed, that on recovering from his injury he had retained none of his memories of the past, and that he had during the intervening years been employed in the hospital as a tailor. In this personality he is docile and tractable and responds only to the name Gervais. Shortly after his awakening, Jean Mourin is given his discharge from the hospital and, like Lazarus of old, he returns to the world as from the dead. He finds that all his friends and acquaintances have died during the years of his absence, and that except for the haunting presence of Gervais, of whom he is fearful and from whom he continually flees, he is utterly alone. His struggles and fears, his thoughts and actions during his hours of freedom are vividly and realistically described. Béraud has woven into an interesting novel the psychological facts of double personality.

Cornell University C. V. BROOK

An Introduction to Objective Psychopathology. By G. V. HAMILTON. St. Louis, C. V. Mosby Co., 1925, pp. 344. With a foreword by ROBERT M. YERKES.

Not many of the numerous books on psychopathology in recent years make a distinct contribution to the subject. Dr. Hamilton's volume is an exception. Although its point of view and its contents cannot startle the world as Freudism has done, it promises to be of more permanent scientific value.

The author's aim, as the title suggests, is to study abnormal behavior from the objective point of view, making use of the methods of comparative and genetic psychology. The results of observation do not form a departure for speculations about an unconscious, but are stated in a form to suggest certain behavior types. One is, however, a little irritated to run across such expressions as "pathologically persistent nonadjustive affective reactions to baffling somatic discomforts and disabilities."

Approximately one-half of the book is devoted to detailed descriptions of the reaction tendencies of 200 nervous patients. A statistical treatment shows that the most frequent abnormal behavior tendencies are "persistent nonadjustive affective reactions" to baffling situations, somatic or social, or "indirect reactions" and "conditioned reactions."

The remaining pages are given to a constructive, systematic treatment of the facts of psychopathology, which the author properly defines as a branch of medicine. Many Freudian mechanisms will be recognised under new names, but names which now refer to observable behaviour, which includes a report of the psychological experiences of the patient, rather than to mystical entities. A report of comparative studies of twelve species of mammals in similar baffling situations is very enlightening, and leads the author to conclude that the best cure for nervousness is to bring the patient to a rational understanding of his symptoms. A final chapter on sex behavior agrees in part with Freud's theories, but in places it conflicts with them. Wherever there is disagreement one is inclined to accept the undisputed facts of comparative behavior. A glossary of medical terms makes the book accessible to the non-medical reader. A bibliography of 126 references is given.

Cornell University J. P. GUILFORD

NOTES

DR. JOHNSON ON THE MEASUREMENT OF ATTENTION

The problem of the measurement of attention is of importance from both a practical and a theoretical point of view; consequently many attempts with various methods have been made to solve it. Geissler,[1] in an historical survey, arranged the methods in 6 groups: "The first 5 may be classed together as methods of expression analogous to the physiological methods employed in the investigation of the affective processes. According to these 5 methods the degrees of attention may express themselves in changes of (1) peripheral vision; (2) muscular strength; (3) liminal and differential sensitivity; (4) reaction-time; (5) accuracy of work. The sixth method may be likened to the method of impression. By a series of graded distractors, different degrees of attention are to be induced in the *O*, and he is afterwards to report which degree he experienced." Not one of these methods has given univocal results, and, for one reason or another, not one of them seems to be adequate to the problem.[2] I am here concerned only with (4),—the method of measuring degree of attention by speed of reaction.

Obersteiner, in 1879,[3] was the first to determine degrees of attention by means of reaction-time. He had two distractors,—an induction current and a music-box,—and he found that under distraction his *Os*' reactions were slower than under normal conditions; "this retardation," he concluded, "stands in inverse proportion to the intensity of attention;"[4] but "the differences in the reaction period, which may serve directly as the measure of attention, vary in different individuals, and in the same individual under different conditions."[5] The results of this first attempt had thus to be qualified. Since Obersteiner, many investigators[6] have tried to obtain an

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[1] L. R. Geissler, The Measurement of Attention, this JOURNAL, 20, 1909, 492.
[2] *Cf.* L. R. Geissler, *op. cit.*, 493-502.
[3] H. Obersteiner, Experimental Researches on Attention, *Brain*, 1, 1879, 439-453.
[4] *Op. cit.*, 444.
[5] *Ibid.*
[6] G. Buccola, La durata del discernimento e della determinazione volitiva, *Rivista di filos. scientif.*, 1, 1881, 19 ff.
G. S. Hall, Reaction-time and Attention in the Hypnotic State, *Mind*, 8, 1883, 170-182.
E. J. Swift, Disturbance of Attention, this JOURNAL, 5, 1892, 1-19.
J. McK. Cattell, The Time taken up by Cerebral Operations, *Mind*, 11, 1886, 220-242; 377-392; 524-538.

objective measure of attention by way of the reaction method; the results have been flatly contradictory.[7]

The results of the other methods are no less equivocal. Consequently, when Geissler turned to the problem of measuring attention, he decided to strike out upon a new tack. He proposed to measure attention in terms of clearness,—the single characteristic of attention upon which all writers, theoretical as well as experimental, seemed to be agreed. This attempt led to positive results. Geissler, however, worked only with material that was visually presented; hence his results were subject to the criticism that they were obtained in a sense-department that had itself a focus and a margin, and was itself differentiated into degrees of optical clearness or distinctness. Before his method could be accepted, and before the conclusion that attention was measurable in terms of attributive clearness or attensity[8] could be drawn, his investigations had to be carried with positive results into other domains of sense.

In 1913, and again in 1916, I extended Geissler's method to the fields of auditory and cutaneous sensation. In the earlier[9] of these two Studies, the *Os* were directed to report all changes, in pitch and intensity, of the tonal stimulus; and in the later,[10] all extensive and intensive changes of the cutaneous stimulus. Description of the changes and subjective estimation of the clearness of content at the times of change were given by the *Os* in their verbal reports. Because a close parallelism was found to exist between the introspectively distinguishable variations of clearness and the accuracy and precision of the work performed at the various levels of clearness, I felt justified in concluding, with Geissler, that "attention can be measured introspectively in terms of attributive clearness."

W. James, *Principles of Psychology*, 1, 1890, 95; 425, 427-434.

S. E. Sharp, Individual Psychology: A Study in Psychological Method, this JOURNAL, 10, 1899, 344, 356 *f.*, 379 ff.

A. Kästner and W. Wirth, Die Bestimmung der Aufmerksamkeitsverteilung innerhalb des Sehfeldes mit Hilfe von Reaktionsversuchen, *Psych. Stud.*, 3, 1907, 361-392; 4, 1908, 139-200.

L. R. Geissler, A Critique of Professor Wirth's Methods of Measurement of Attention, this JOURNAL, 20, 1909, 126 ff.; The Measurement of Attention, *ibid.*, 508 f., 514.

I. Grassi, Einfache Reaktionszeit und Einstellung der Aufmerksamkeit, *Zeits. f. Psych.*, 60, 1911, 46-72.

J. V. Breitweiser, Attention and Movement in Reaction Time, *Arch. of Psych.*, No. 18, 1911, 1-48.

K. M. Dallenbach, The Measurement of Attention, this JOURNAL, 24, 1913, 483 ff., 501 ff.

H. Woodrow, The Measurement of Attention, *Psych. Mon.*, 17, 1914, No. 76, 1-158; Reactions to the Cessation of Stimuli and their Nervous Mechanism, *Psych. Rev.*, 22, 1915, 444 *f.*; Outline as a Condition of Attention, *Jour. Exp. Psych.*, 1, 1916, 23-39.

K. M. Dallenbach, The Measurement of Attention in the Field of Cutaneous Sensation, this JOURNAL, 27, 1916, 453 ff.

E. E. Cassel and K. M. Dallenbach, An Objective Measure of Attributive Clearness, this JOURNAL, 29, 1918, 205 ff.

F. L. Wells, C. M. Keller and G. Murphy, On Attention and Simple Reaction, *Jour. Exp. Psych.*, 4, 1921, 391-398.

J. C. McComas, A Measure of Attention, *Jour. Exp. Psych.*, 5, 1922, 1-18.

H. M. Johnson, Reaction-Time Measurements, *Psych. Bull.*, 20, 1923, 578-580.

K. M. Dallenbach, Dr. Johnson on Reaction-Time Experiments, this JOURNAL, 35, 1924, 305-307.

H. M. Johnson, The Definition and Measurement of Attention, this JOURNAL, 36, 1925, 604-613.

[7]Cattell, in 1886 (*op. cit.*, 242), found that there was no corresponding lengthening of the reaction-time with reduction of attention. Geissler, in 1909 (*op. cit.*, 498), wrote, "the final outcome of the reaction experiments, as used for the measurement of attention, has been on the whole negative; it has been impossible to establish a positive correlation between high degrees of attention and short reactions, and between low degrees and correspondingly lengthened reactions." Woodrow, in 1914 (*op. cit.*, 139), said, "that reaction time may vary without corresponding variation in attention." Johnson, in 1925 (*op. cit.*, 612), asserted, "that knowledge of a particular reaction-time . . . . . does not enable prediction . . . . . of the unknown clearnesses associated with them."

[8]E. B. Titchener, The Term "Attensity", this JOURNAL, 35, 1924, 156.

[9]*Op. cit.*, 474, 492.

[10]*Op. cit.*, 443-445.

In both Studies graphic records were made of the objective results. One line on the kymograph showed the time of change; a second line, *O*'s judgment of that change. One 'tick' in the line denoted a change of intensity; two 'ticks', a change of pitch (in the first Study) or of extent (in the second Study). A Jacquet chronoscope, writing fifths of seconds, was placed between these two lines, and reaction-times were taken directly from the records.

What to do with these 'reaction-times' after they had been calculated was a question! To ignore them would, it seemed to me, be unscientific; to compute, from the scatter-graph, the correlation ratios between the gross measures would, after the failure of the earlier attempts, be futile. I had another choice,—to work the data over from a new angle. Rather than discard the data, or repeat a method that had proved useless, I decided to draw a correlation between degree of clearness and average reaction-time. I based the procedure of averaging on the theory that clearness was not the only determinant of the length of the simple sensory reaction, that many other unknown and adventitious conditions were also effective, and that the averaging of the reaction times that were paired with the various degrees of clearness would cancel out the effect of all the uncontrolled determinants and would thus reveal the true relation between clearness (the only condition whose variations were known) and reaction.

"The *average reaction-time* was computed for each degree of attention as estimated by the clearness of mental processes,"[11] and the correlation was computed between these two arrays. Though the correlations were high, I qualified my conclusion "that the average reaction-time would index attention," because my *O*s were not working under reaction instructions;[12] they did not know that their reactions were being measured, and so they were not under the compulsion to react as soon as possible after perceiving a change. Indeed, as I pointed out in both Studies, the *O*s at times delayed their reactions;—particularly "if the change occurred at a critical point they might voluntarily delay their reaction until a more favorable opportunity to react presented itself."

Since my times were not "reaction-times" in the true sense of the term, I was glad, while working with Miss Cassell in 1918,[13] to avail myself of the opportunity of testing the average reaction-time method and the proposition that clearness and length of sensory reaction are related.[14]

Our experiment fell into two parts. In the first part, *O* estimated, after every series of 10 reactions, the clearness of the mental processes during that period. Under our conditions, the average time of the simple sensory reaction was inversely correlated with the introspectively reported clearness-values. The results, however, were open to two obvious objections: "(1) that *O*'s reports, inasmuch as they were delayed until after the completion of 10 reactions, partake more of the nature of vague impressions, of guesses, than of true introspective descriptions; and (2) that the figures, inasmuch as they are averages of averages, are artificial and not representative."[15] To meet these objections and further to check the method and results, *O*, in the second part of the experiment, reported immediately after every reaction (700 in all) the clearness of the mental processes during the reaction. Correlation was computed as before between the different clearness values reported and the averages of the reaction-times for the different clearnesses. Since the results of the two parts of the experiment agreed, and the correlations thus obtained were high and persisted through marked shifts of attitude, we felt justified in concluding that "unde rour conditions of training, attributive clearness may be measured by the average duration

[11] *Op. cit.*, 1913, 501. Italics in the original. [12] *Opp. cit.*, 1913, 485, 502; 1916, 455 f.
[13] E. E. Cassel and K. M. Dallenbach, The Effect of Auditory Distraction upon the Sensory Reaction, this JOURNAL, 29, 1918, 129-143.
[14] E. E. Cassell and K. M. Dallenbach, An Objective Measure of Clearness, this JOURNAL, 29, 1918, 205.
[15] *Ibid.*, 206.

and mean variation of the simple sensory reaction."[16] We explicitly forebore generalization, as the following sentence from our conclusion shows, "Since they [the results] were obtained from a single *O*, who was highly practiced both in reaction and in the introspection of clearness, we have no right to generalize."

In his review of reaction-time experiments, Dr. Johnson[17] considered our experiment, and criticised us for not "treating each paired measurement as a single datum."[18] Such treatment would be futile, as Dr. Johnson himself shows by obtaining from our data a correlation that is only "3 percent more reliable than guesswork,"[19] and as any one conversant with the literature on the problem knows. Regarding our correlation between clearness and average reaction-time, Dr. Johnson says, "such treatment disregards the variations of time corresponding to each degree of clearness."[20] Yes; as I pointed out in my Note written in reply to his criticisms,[21] it was just such variations that we wished to disregard or to eliminate by averaging. "We thought, by averaging the reaction-times that were paired with the various degrees of clearness, to eliminate the effect of the other conditions (unknown and uncontrolled) and thus to discover the relations that existed between clearness and reaction."[22] Against this procedure he raised no objection; his criticisms were therefore irrelevant,—he missed the point of the experiment entirely.

In his rejoinder,[23] Dr. Johnson (*1*) accuses me "of abandoning the proposition" that "reaction-time is closely correlated with clearness;" and (*2*) complains that he does not know "the meaning of the expression 'average duration' or 'average reaction-time,' "[24] *i.e.*, that he does not understand our method. Regarding (*1*) he is in error; we never maintained that proposition. From our knowledge of the earlier work we knew that the proposition was untenable. Every reference, every conclusion, explicit or implied, referred to the *average* reaction-time as the measure of clearness. Averaging was the very 'essence' of our method. How this escaped him I am at a loss to know, for 'average' appears as a qualifying word in our table captions and in our conclusions. The only place in which it does not appear is in a footnote; but this should not have confused him as the sentence to which the footnote was appended contained the specific qualification.[25] Regarding (*2*): if the meaning of 'average duration' or 'average reaction-time' is not clear from Miss Cassell's and my report, it should have been clear from the exposition of the method given in my Note, where I said, "We give, in Table II, (1) the different clearness values reported, (2) the frequency of their report, (3) the average reaction times for the different clearnesses, and (4) the mean variations of these averages. We computed the correlations, . . . between clearness and average reaction-time, *i.e.*, between (1) and (3)."[26] Yet he complains that he is unable to understand what we did! He asks: Does average reaction-time "refer specifically to the means of times which were associated with specific class-values of clearness [restricted sense]; or does it refer to the average of times taken without regard to the numerical values of clearness associated with them [unrestricted sense]?"[27] Because he is unable to decide which is meant he examines both; and shows that the 'unrestricted sense' is untenable. With this argument, as with his previous mathematical argument, I am in substantial agreement,—which is to say that I regard it as irrelevant.

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[16] *Ibid.*, 207.
[17] H. M. Johnson, Reaction-time Measurements, *Psych. Bull.*, 20, 1913, 562-589.
[18] *Op. cit.*, 570. [19] *Op. cit.*, 580. [20] *Op. cit.*, 570.
[21] K. M. Dallenbach, Dr. Johnson on Reaction-time Experiments, this JOURNAL, 35, 1924, 307.
[22] *Ibid.*
[23] H. M. Johnson, Definition and Measurement of Attention, this JOURNAL, 36, 1925, 601-615.
[24] *Op. cit.*, 608. [25] *Op. cit.*, 207 [26] *Op. cit.*, 306. [27] *Op. cit.*, 608 *f.*

Though he admits the truth of our conclusion, that "attributive clearness may be measured by the *average* duration of the simple sensory reaction," if 'average duration' be taken in the 'restricted sense,' he nevertheless raises the following objections against it. "(1) It is without practical significance,"—with its practicality we were not at all concerned. "(2) The assertion [our conclusion] is misleading since the restricted meaning is not denoted,"—but we did denote the restricted meaning. "(3) The assertion [our conclusion], taken literally, is irrelevant to a number of passages in the article with which it was connected."[28] Of the 3 passages that Dr. Johnson quotes as illustrative of this objection the first is an excerpt from a footnote and is out of its contextual setting; the second is relevant for it contains the very words of our conclusion; and the third is qualified by the phrase "under our conditions," which refers to method as well as to procedure; none of his illustrations sustains his criticism.

In my Note, I said that Dr. Johnson was not always accurate in quotation and report, and I gave instances.[29] In his rejoinder he repeats the offense. In the first place, he confuses cognitive and attributive clearness, and makes "degree of consciousness" synonymous with "clearness of content;"[30] and in the second place, his statements regarding our method do not accord with the facts. "Experienced introspectors," he writes, "assert that the task of comparing the clearnesses of various portions of content, independently of their other characteristics, is not easy. The assignment of satisfactory numerical values to these clearnesses is still more difficult. After long practice, a few adepts in the art of introspection achieve . . . consistency."[31] In none of the experiments with this method have the *O*s complained of any difficulty. On the contrary, they found it as a technical method unusually easy to master. Geissler's *O*s, "both experienced and inexperienced, found it relatively easy;" "after 8 or 10 hrs. of practice the *O*s had worked out for themselves a scale of 9 degrees of clearness."[32] Rogers found after "2 series of preliminary experiments" that her *O*s were able to estimate "the various processes according to 9 degrees of clearness."[33] In my experiments with the same method I have never met with any difficulty.[34] In all, 36 *O*s which is more than a few, have become adept in this "art" of observation. Not one of those who took part in these experiments was, to my knowledge, excused from the work because of difficulties with the method.

To the charge of inaccuracy, I must now add the charge of inconsistency. In his Review, Dr. Johnson claimed that "the introspective findings might, of course, be challenged."[35] In his rejoinder, he maintains that he has 'never been seriously inclined to challenge the concreteness or the appropriateness of the introspective clearness values or the reaction-times.'[36]

Dr. Johnson gives our results an interpretation of his own: "*The reaction-time is determined chiefly, if not entirely, by the degree of adjustment*

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[28] *Ibid.*

[29] *Op. cit.*, 307. As an example of his inaccuracies I gave the following: "Two flashes of a Geissler tube serving as preparatory signals were given in succession 3″ and 1.8″ respectively, before the stimulus." I pointed out that we did not use a Geissler tube, but a "2.9 volt electric globe," and the preparatory signals occurred "3 sec. and 1½ sec. before the occurrence of the stimulus." In the rejoinder Dr. Johnson says that the first error was a copyist's substitution, and that the second was not an error of report on his part as the preparatory signals actually occurred "as timed by the Hipp chronoscope $3265 \pm 32.6\,\sigma$ and $1853 \pm 11.8\,\sigma$ respectively before the stimulus." If he makes the correction in the time of the second signal which differs from the true setting by 0.35 sec., should he not have, by the same token, made a correction in the time of the first signal, which, as reported, differed from the true setting by almost the same amount, 0.26 sec.? Or does his "liminal threshold for accuracy" fall between these two values?

[30] *Op. cit.*, 601. *Cf.* E. B. Titchener, The Psychological Concept of Clearness, *Psych. Rev.*, 24, 1917, 48 ff.

[31] *Op. cit.*, 601. [32] *Op. cit.*, 511.

[33] A. S. Rogers, An Analytic Study of Visual Perceptions, this JOURNAL, 28, 1917, 558 f.

[34] *Op. cit.*, 467. [35] *Op. cit.*, 578. [36] *Op. cit.*, 606.

for reception and response; and hence indicates the degree of 'attention' in the objective meaning of that term."[17] The value of this interpretation becomes evident when we recall that in muscular, sensory, choice, or associative reactions, the degree of attention may be the same while the reaction-times differ greatly.

Cornell University K. M. DALLENBACH

EXPERIMENTAL PSYCHOLOGY IN THE ORIENT

Notable progress in the development of laboratories for psychology in the Far East is indicated by the following bit of information, most of it supplied to the JOURNAL by Mr. W. Wideman, Secretary to the C. H. Stoelting Company, Chicago. It appears that many of the Eastern universities have been coming to America since the War for psychological equipment as well as for 'testing' supplies. Among them are the University of Calcutta (Dr. N. N. Sengupta); Maharaja's College, Mysore (Dr. M. V. Gopalaswami); Dacca University (H. D. Bhattachharyya); Brahmamavidyashrama ("School of Universal Knowledge") Adyar, Madras; National University of Kwangtung, Canton, China (Dr. Hishing Wong); Teachers College, Wuchang (Professor F. B. Cheng); Fuh Tan University, Shanghai (which has recently built a psychological laboratory for Dr. Zing Yang Kuo, formerly of the University of California); Peking Government Teachers College (Professor Y. C. Chang); Amoy University (Dr. C. H. Chou); National Southeastern University, Nanking (Dr. C. W. Luh); National University of Peking (Dr. T. T. Lew); Amoy University; Kyoto Imperial University, Japan; College of Commerce, Nagoya (Professor Y. Kaga); and Tokyo School for Mental Deficiency (Dr. R. Ishii).

Industrial applications are well on the way, at least in Japan. It is reported that a Department of Psychology is maintained in one of the spinning centers, and the "Institute for Science of Labor" at Kurasiki, has just now published a report, which includes a bibliography of about twenty Japanese references and recites four years' work of the six departments of the Institute.—The *Japanese Journal of Psychology* (published in Japanese) is now in its third year. It has published a number of experimental articles.

University of Illinois MADISON BENTLEY

IMPROVEMENTS ON THE SANFORD VERNIER CHRONOSCOPE

Two improvements on the Sanford Vernier Chronoscope have been worked out by Mr. John R. Roberts, mechanician of the Psychological Laboratory at the University of Pennsylvania. (1) The method of fastening and adjusting the threads that hold the bobs was improved. In the older type of Sanford apparatus the threads were wound about a washer on the horizontal bar. Exact adjustment of the length of the bobs was difficult because the threads had to be unwound at every change. The improvement was made as follows. A hole was drilled in the horizontal bar between the two thread-holes and reamed to a taper. Into this a taper pin was fitted. A slot was cut in the base of the pin and a hole drilled through the side near the base. The thread was then passed through the bob, through the holes in the horizontal bar, and finally through the hole in the taper pin and there tied. When the taper pin is turned, as one turns the key of a violin, the length of the threads is changed. Adjustment is easy Twisting the taper pin brings up both sides evenly so that the bob always

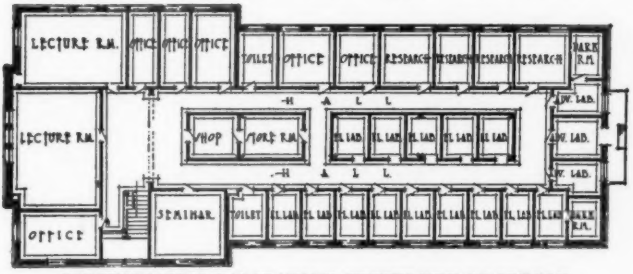
[17] *Op. cit.*, 605. Italics in the original.

hangs in its proper position; it is not canted. (2) The second improvement has to do with the holder for visual stimuli. On the older type of chronoscope the visual stimulus had to be inserted in a clip fastened to the horizontal bar. In the case of choice reactions, in the laboratory drill section, the changing of the stimulus took a great deal of time. On the bar that carries the clip, we have now mounted a small slotted frame, both side of which are open. This is on a swivel so it can be rotated through 180°. The true and false stimuli are placed back to back in the frame and the exposure of either stimulus is effected by a mere turn.

University of Pennsylvania SAMUEL W. FERNBERGER

THE NEW PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF OREGON

The department of psychology of the University of Oregon, now occupies its new quarters on the top floor of the new science building. Fifteen small rooms are devoted to elementary laboratory. Every room is furnished with a cabinet in which there is a year's supply of materials for one pair of students. Four rooms, including a dark room, are reserved for the advanced laboratory, four rooms for graduate students, six offices for members of the staff, two lecture rooms for classes not over 70, a combined seminary room and departmental library, a supply room and a shop.



PSYCHOLOGICAL LABORATORY, UNIVERSITY OF OREGON

With the exception of the rooms in the center of the hall,—which are temporary structures to be torn down if desired when the next unit of the science building is constructed and the department expands on the same floor,—all rooms are supplied with direct and alternating currents and an intercommunicating system of wires. Every room has compressed air and gas. The department has its own generator, batteries and air-pump. The laboratory is relatively sound-proof in construction, with staggered sills, padded joints, lined walls and floors, and battleship linoleum in all except the lecture room.

University of Oregon E. S. CONKLIN

EIGHTH INTERNATIONAL CONGRESS OF PSYCHOLOGY: SECOND ANNOUNCEMENT

The second announcement of the eighth international congress of psychology, which will be held at Groningen, Holland, Sept. 6-11, 1926, gives the following provisional list of symposia and speakers: *Understanding and Explanation* by Dr. Binswanger of Kreuzlingen, Dr. Jaspers of

Heidelberg, and Dr. Spranger of Berlin; *Intensity of Sensations* by Dr. Boring of Harvard, Dr. Myers of London, and Dr. Werner of Hamburg; *Form-Qualities* by Dr. Benussi of Turin, Dr. Koffka of Giessen, and Dr. Michotte of Louvain; *Behaviorism* by Dr. McDougall of Harvard and Dr. Piéron of Le Vésinet; *Psychology of the Primitive Races* by Dr. Bartlett of Cambridge, Dr. Levy-Bruhl of Paris, Dr. Mayer-Gross of Heidelberg, and Dr. Storch of Tübingen; and *Religious Psychology* by Dr. Janet of Paris and Dr. Leuba of Bryn Mawr.

Other papers have been promised by Dr. Baumgarten of Solothurn, Dr. and Mrs. Bühler of Vienna, Dr. Buyse of Louvain, Dr. Foucault of Montpellier, Dr. Jaensch of Marburg, Dr. Julász of Budapest, Dr. Ley of Brussels, Dr. Marbe of Würzburg, Dr. Mayer-Gross of Heidelberg, Dr. Paulhan of Paris, Dr. Peters of Jena, Dr. Rignano of Milan, Dr. Rupp of Berlin, Dr. Selz of Mannheim, Dr. Spearman of London, Dr. Stern of Hamburg, Dr. Thurstone of New York and Dr. Werner of Hamburg.

Psychologists who wish to attend and have not received an invitation should communicate with the First Secretary, Professor Dr. F. Roels, Maliebaan 86, Utrecht, Holland. Membership subscription is 15 gulden.

Psychologie und Medizin

We have just received the first number (October, 1925) of *Psychologie und Medizin, Vierteljahrsschrift für Forschung und Anwendung auf ihren Grenzgebieten*, the official organ of the *Psychologische Gesellschaft* and the *Arbeitsgemeinschaft für praktische Psychologie* of Berlin. The Journal is edited by Dr. R. W. Schulte of Berlin with the cooperation of Professor R. Sommer of Giessen, Professor Erich R. Jaensch of Marburg, Dr. Albert Moll of Berlin, Professor W. Wirth of Leipzig, and Professor R. H. Goldschmidt of Münster.

A prefatory note by the editors and the following original articles appear in this number: R. W. Schulte, *Psychologie und Medizin;* R. H. Goldschmidt, *Psychologische Grundbegriffe für die ärztliche Begutachtung;* Robert Sommer, *Psychologie und Organisation des Erfinderwesens;* F R. Jaensch, *Ueber eine Lücke im gegenwärtigen Wissenschaftsbetrieb und das Verhältnis von Psychologie und Medizin;* Albert Moll, *Okkultismus und Psychologie;* Fritz Giese, *Zum Begriff der Kulturpathologie;* Hans Friedenthal, *Die Rolle der Physiognomik in der menschlichen Praxis;* J. H. Schultz, *Zur Psychologie der Homosexualität;* R. W. Schulte, *Ueber Elektrodiagnose seelischer Eigenschaften;* G. A. Roemer, *Atmung und musikalisches Erleben;* and Georg Villwock, *Werkstattarbeit und Muskelphysiologie.* Besides these articles the initial number contains reports of the meetings of the *Deutscher Verein für Psychiatrie* and of the *Gesellschaft Deutscher Nervenärzte* (Kassel, September, 1925); as well as the proceedings of the Second International Congress for Individual Psychology held at Berlin. Notes.

K. M. D.

ERRATA

In the article on "The Acquisition of Absolute Pitch," by Helen K. Mull, which appeared in the October, 1925, number of the JOURNAL, the following corrections should be made: p. 486, line 16, for "5," read "0.5;" p. 486, line 17, for "75," read "0.75;" p. 488, line 42, for "g" read "g#" in both cases.

E. G. B.

In my article on "Definition and Measurement of Attention," which appeared in the October, 1925, number, the following corrections should be made: on p. 610, Definition (14), for "$f\sigma_m\sigma_m$," read "$f\sigma_{m_x}\sigma_{m_y}$;" on p. 611, Equation (III), for "σ_{mx}/σ_{my}," read "σ_{my}/σ_{mx};" the same correction should be made also in the third line below this; and on p. 613, footnote, line 1, for "longer," read "shorter."

H. M. JOHNSON